PROCEEDINGS

OF

HELD AT

COLUMBUS, OHIO.

AUGUST, 1899.

PROCEEDINGS

OF

The American Association

FOR THE

ADVANCEMENT OF SCIENCE

FORTY-EIGHTH MEETING

HELD AT

COLUMBUS, OHIO.

AUGUST, 1899.

2602

EDITED BY

L. O. HOWARD, *Permanent Secretary,*

Cosmos Club, Washington, D. C.
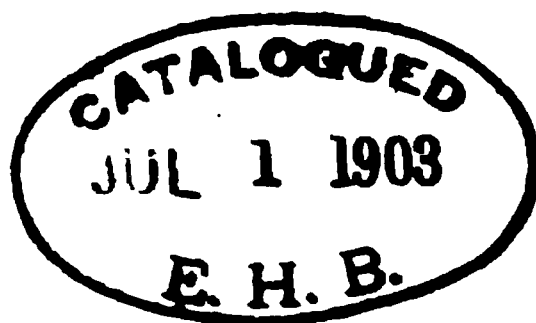
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Ohio State University, Columbus, Ohio.

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E.   Geology and Geography—ARTHUR HOLLICK, Columbia University, New York, N. Y.

F.   Zoology—C. L. MARLATT, Washington, D. C. (In place of F. W. True, resigned.)

G.   Botany—W. A. KELLERMAN, Ohio State University, Columbus, Ohio.

H.   Anthropology—E. W. SCRIPTURE, Yale University, New Haven, Conn. (In place of GEORGE A. DORSEY, resigned.)

I.   Social and Economic Science—CALVIN M. WOODWARD, Washington University, St. Louis, Mo.

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(8)

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G. D. T. MacDOUGAL, Columbia University.
H. FRANK RUSSELL, Harvard University.
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Special Committees of the Association.[1]

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1. *Auditors.*

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2. *Committee on Indexing Chemical Literature.*

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6. *Committee for the Study of the White Race in America.*

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8. *Committee for the Collection of Information Relative to Forestry.*

W. H. BREWER, *Chairman*, GIFFORD PINCHOT, ARNOLD HAGUE.

9. *Committee on the Increase of the Efficiency of the U. S. Naval Observatory.*

[Vacant.]

10. *Committee on the Quantitative Study of Biological Variation.*

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[1] All Committees are expected to present their reports to the COUNCIL not later than the third day of the meeting. Committees sending their reports to the Permanent Secretary one month before a meeting can have them printed for use at the meeting.

MEETINGS AND OFFICERS OF THE ASSOCIATION OF AMERICAN GEOLOGISTS AND NATURALISTS.

Meeting.	Date.	Place.			Assist. Secretary.	Treasurer.
1st	April 2, 1840,	Philadelphi	Benj. Silliman,*	L. C. Beck,*	{ B. Silliman, Jr.,* C. B. Trego,* J. D. Whitney,* M. B. Williams,*	
2d	April 5, 1841,	Philadelphia,	S. G. Morton,*	C. T. Jackson,*		
3d	April 25, 1842,	Boston,	Henry D. Rogers,*	B. Silliman, Jr.,*		John Locke.*
4th	April 26, 1843,	Albany,	John Locke,*	{ B. Silliman, Jr.,* O. P. Hubbard,		Douglas Houghton*
5th	May 8, 1844,	Washington,	Wm. B. Rogers,*	{ B. Silliman, Jr.,* J. Law. Smith,*		Douglas Houghton*
6th	April 30, 1845,	New Haven,	C. T. Jackson,*	B. Silliman, Jr.,*		E. C. Herrick,*
7th	Sept. 2, 1846,	New York,				
8th	Sept. 20, 1847,	Boston,	Wm. B. Rogers,†	Jeffries Wyman,*		B. Silliman, Jr.*

* Deceased.

† Professor Rogers, as chairman of this last meeting, called the first meeting of the new Association to order and presided until it was fully organized by the adoption of a constitution. As he was thus the first presiding officer of the new Association, it was directed at the Hartford meeting that his name be placed at the head of the Past Presidents of the American Association for the Advancement of Science.

Meetings.	Place.	Date.	Members in attendance.	Number of members.
1	Philadelphia	Sept. 20, 1848	?	461
2	Cambridge	Aug. 14, 1849	?	540
3	Charleston	Mar. 12, 1850	?	622
4	New Haven	Aug. 19, 1850	?	704
5	Cincinnati	May 5, 1851	87	800
6	Albany	Aug. 19, 1851	194	769
7	Cleveland	July 28, 1853	?	940
8	Washington	April 26, 1854	168	1004
9	Providence	Aug. 15, 1855	166	605
10	2nd Albany	Aug. 20, 1856	381	722
11	Montreal	Aug. 12, 1857	351	946
12	Baltimore	April 28, 1858	190	962
13	Springfield	Aug. 3, 1859	190	862
14	Newport	Aug. 1, 1860	135	644
15	Buffalo	Aug. 15, 1866	79	637
16	Burlington	Aug. 21, 1867	73	415
17	Chicago	Aug. 5, 1868	259	686
18	Salem	Aug. 18, 1869	244	511
19	Troy	Aug. 17, 1870	188	536
20	Indianapolis	Aug. 16, 1871	196	668
21	Dubuque	Aug. 15, 1872	164	610
22	Portland	Aug. 20, 1873	195	670
23	Hartford	Aug. 12, 1874	224	722
24	Detroit	Aug. 11, 1875	165	807
25	2nd Buffalo	Aug. 23, 1876	215	867
26	Nashville	Aug. 29, 1877	173	953
27	St. Louis	Aug. 21, 1878	134	962
28	Saratoga	Aug. 27, 1879	256	1030
29	Boston	Aug. 25, 1880	997	1555
30	2nd Cincinnati	Aug. 17, 1881	500	1699
31	2nd Montreal	Aug. 23, 1882	937	1922
32	Minneapolis	Aug. 15, 1883	328	2033
33	2nd Philadelphia	Sept. 3, 1884	1261*	1981
34	Ann Arbor	Aug. 26, 1885	364	1956
35	3d Buffalo	Aug. 18, 1886	445	1886
36	New York	Aug. 10, 1887	729	1956
37	2nd Cleveland	Aug. 14, 1888	342	1964
38	Toronto	Aug. 26, 1889	424	1952
39	2d Indianapolis	Aug. 19, 1890	364	1944
40	2d Washington	Aug. 19, 1891	653†	2054
41	Rochester	Aug. 17, 1892	456	2037
42	Madison	Aug. 17, 1893	290	1939
43	Brooklyn	Aug. 15, 1894	488	1802
44	2d Springfield	Aug. 28, 1895	368	1913
45	4th Buffalo	Aug. 24, 1896	333	1890
46	2d Detroit	Aug. 9, 1897	283‡	1782
47	2d Boston	Aug. 22, 1898	903	1729
48	Columbus	Aug. 21, 1899	353	——
49	2d New York	June 25, 1900	——	——

* Including 303 members of the British Association and 9 other foreign guests.
† Including 24 Foreign Honorary members for the meeting.
‡ Including 15 Foreign Honorary members and associates for the meeting.

Officers of the Meetings of the Association.

[The number before the name is that of the meeting ; the year of the meeting follows the name ; the asterisk after a name indicates that the member is deceased.]

PRESIDENTS.

1. { WM. B. ROGERS,* 1848.
   { W. C. REDFIELD,* 1848.
2. JOSEPH HENRY,* 1849.
3, 4, 5. A. D. BACHE,* March meeting, 1850, in absence of JOSEPH HENRY.* August meeting, 1850. May meeting, 1851.
6. LOUIS AGASSIZ,* August meeting, 1851.
   (No meeting in 1852.)
7. BENJAMIN PIERCE,* 1853.
8. JAMES D. DANA,* 1854.
9. JOHN TORREY,* 1855.
10. JAMES HALL,* 1856.
11, 12. ALEXIS CASWELL,* 1857, in place of J. W. BAILEY,* deceased. 1858, in absence of JEFFRIES WYMAN.*
13. STEPHEN ALEXANDER,* 1859.
14. ISAAC LEA,* 1860.
    (No meetings for 1861-65.)
15. F. A. P. BARNARD,* 1866.
16. J. S. NEWBERRY,* 1867.
17. B. A. GOULD,* 1868.
18. J. W. FOSTER,* 1869.
19. T. STERRY HUNT,* 1870, in the absence of WM. CHAUVENET.*
20. ASA GRAY,* 1871.
21. J. LAWRENCE SMITH,* 1872.
22. JOSEPH LOVERING,* 1873.
23. J. L. LECONTE,* 1874.
24. J. E. HILGARD,* 1875.
25. WILLIAM B. ROGERS,* 1876.

26. SIMON NEWCOMB, 1877.
27. O. C. MARSH,* 1878.
28. G. F. BARKER, 1879.
29. LEWIS H. MORGAN,* 1880.
30. G. J. BRUSH, 1881.
31. J. W. DAWSON, 1882.
32. C. A. YOUNG, 1883.
33. J. P. LESLEY, 1884.
34. H. A. NEWTON,* 1885.
35. EDWARD S. MORSE, 1886.
36. S. P. LANGLEY, 1887.
37. J. W. POWELL, 1888.
38. T. C. MENDENHALL, 1889.
39. G. LINCOLN GOODALE, 1890.
40. ALBERT B. PRESCOTT, 1891.
41. JOSEPH LECONTE, 1892.
42. WILLIAM HARKNESS, 1893.
43. DANIEL G. BRINTON,* 1894.
44. E. W. MORLEY, 1895.
45. { EDWARD D. COPE,* 1896.
     { THEODORE GILL, as senior vice president acted after the death of Prof. COPE.
46. WOLCOTT GIBBS, 1897, absent. W J McGEE, Acting President.
47. F. W. PUTNAM, 1898.
48. EDWARD ORTON,* 1899.
49. R. S. WOODWARD, 1900.

VICE PRESIDENTS.

There were no Vice Presidents until the 11th meeting when there was a single Vice President for each meeting. At the 24th meeting the Association met in Sections A and B, each presided over by a Vice President. At the 31st meeting nine sections were organized, each with a Vice President as its presiding officer. In 1886, Section G (Microscopy) was given up. In 1892, Section F was divided into F, Zoology ; G, Botany.

1857-1874.

11. ALEXIS CASWELL,* 1857, acted as President.
12. JOHN E. HOLBROOK,* 1858, not present.
13. EDWARD HITCHCOCK,* 1859.
14. B. A. GOULD,* 1860.
15. B. A. GOULD,* 1866, in absence of R. W. GIBBES.
16. WOLCOTT GIBBS, 1867.

17. CHARLES WHITTLESEY,* 1868.
18. OGDEN N. ROOD, 1869.
19. T. STERRY HUNT,* 1870, acted as President.
20. G. F. BARKER, 1871.
21. ALEXANDER WINCHELL,*1872.
22. A. H. WORTHEN,* 1873, not present.
23. C. S. LYMAN,* 1874.

1875-1881.

Section A.—Mathematics, Physics, and Chemistry.
24. H. A. NEWTON,* 1875.
25. C. A. YOUNG, 1876.
26. R. H. THURSTON, 1877, in the absence of E. C. PICKERING.
27. R. H. THURSTON, 1878.
28. S. P. LANGLEY, 1879.
29. ASAPH HALL, 1880.
30. WILLIAM HARKNESS, 1881, in the absence of A. M. MAYER.*

Section B.—Natural History.
24. J. W. DAWSON, 1875.
25. EDWARD S. MORSE, 1876.
26. O. C. MARSH,* 1877.
27. AUG. R. GROTE, 1878.
28. J. W. POWELL, 1879.
29. ALEXANDER AGASSIZ, 1880.
30. EDWARD T. COX, 1881, in the absence of GEORGE ENGELMANN.*

CHAIRMEN OF SUBSECTIONS, 1875-1881.

Subsection of Chemistry.
24. S. W. JOHNSON, 1875.
25. G. F. BARKER, 1876.
26. N. T. LUPTON,* 1877.
27. F. W. CLARKE, 1878.
28. F. W. CLARKE, 1879, in the absence of IRA REMSEN.
29. J. M. ORDWAY, 1880.
30. G. C. CALDWELL, 1881, in the absence of W. R. NICHOLS.*

Subsection of Microscopy.
25. R. H. WARD, 1876.
26. R. H. WARD, 1877.
27. R. H. WARD, 1878, in the absence of G. S. BLACKIE.*

28. E. W. MORLEY, 1879.
29. S. A. LATTIMORE, 1880.
30. A. B. HERVEY, 1881.

Subsection of Anthropology.
24. LEWIS H. MORGAN,* 1875.
25. LEWIS H. MORGAN,* 1876.
26. DANIEL WILSON,* 1877, not present.
27. United with Section B.
28. DANIEL WILSON,* 1879.
29. J. W. POWELL, 1880.
30. GARRICK MALLERY,* 1881.

Subsection of Entomology.
30. J. G. MORRIS,* 1881.

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VICE PRESIDENTS OF SECTIONS, 1882-

Section A.—Mathematics and Astronomy.

31. W. A. ROGERS,* 1882, in the absence of WM. HARKNESS.
32. W. A. ROGERS, 1883.
33. H. T. EDDY, 1884.
34. WM. HARKNESS, 1885,. in the absence of J. M. VAN VLECK.
35. J. W. GIBBS, 1886.
36. J. R. EASTMAN, 1887, in place of W. FERREL,* resigned.
37. ORMOND STONE, 1888.
38. R. S. WOODWARD, 1889.
39. S. C. CHANDLER, 1890.
40. E. W. HYDE, 1891.
41. J. R. EASTMAN, 1892.
42. C. L. DOOLITTLE, 1893.
43. { G. C. COMSTOCK, 1894.
     { EDGAR FRISBY, 1894.
44. EDGAR FRISBY, 1895, in place of E. H. HOLDEN, resigned.
45. ALEX. MACFARLANE, 1896, in place of WM. E. STORY, resigned.
46. W. W. BEMAN, 1897.
47. E. E. BARNARD, 1898.
48. ALEX. MACFARLANE, 1899.
49. ASAPH HALL, JR., 1900.

Section B.—Physics.

31. T. C. MENDENHALL, 1882.
32. H. A. ROWLAND, 1883.
33. J. TROWBRIDGE, 1884.
34. S. P. LANGLEY, 1885, in place of C. F. BRACKETT, resigned.
35. C. F. BRACKETT, 1886.
36. W. A. ANTHONY, 1887.
37. A. A. MICHELSON, 1888.
38. H. S. CARHART, 1889.
39. CLEVELAND ABBE, 1890.
40. F. E. NIPHER, 1891.
41. B. F. THOMAS, 1892.
42. E. L. NICHOLS, 1893.
43. WM. A. ROGERS, 1894.
44. W. LeCONTE STEVENS, 1895.
45. CARL LEO MEES, 1896.

46. CARL BARUS, 1897.
47. F. P. WHITMAN, 1898.
48. ELIHU THOMSON, 1899.
49. ERNEST MERRITT, 1900.

Section C.—Chemistry.

31. H. C. BOLTON, 1882.
32. E. W. MORLEY, 1883.
33. J. W. LANGLEY, 1884.
34. N. T. LUPTON,*, 1885, in absence of W. R. NICHOLS.*
35. H. W. WILEY, 1886.
36. A. B. PRESCOTT, 1887.
37. C. E. MUNROE, 1888.
38. W. L. DUDLEY, 1889.
39. R. B. WARDER, 1890.
40. R. C. KEDZIE, 1891.
41. ALFRED SPRINGER, 1892.
42. EDWARD HART, 1893.
43. T. H. NORTON, 1894.
44. WM. McMURTRIE, 1895.
45. W. A. NOYES, 1896.
46. W. P. MASON, 1897.
47. EDGAR F. SMITH, 1898.
48. F. P. VENABLE, 1899.
49. JAS. LEWIS HOWE, 1900.

Section D.—Mechanical Science and Engineering.

31. W. P. TROWBRIDGE,* 1882.
32. DE VOLSON WOOD, 1883, absent, but place was not filled.
33. R. H. THURSTON, 1884.
34. J. BURKITT WEBB, 1885.
35. O. CHANUTE, 1886.
36. E. B. COXE, 1887.
37. C. J. H. WOODBURY, 1888.
38. JAMES E. DENTON, 1889.
39. JAMES E. DENTON, 1890, in place of A. BEARDSLEY, absent.
40. THOMAS GRAY, 1891.
41. J. B. JOHNSON, 1892.
42. S. W. ROBINSON, 1893.
43. MANSFIELD MERRIMAN, 1894.
44. WILLIAM KENT, 1895.
45. FRANK O. MARVIN, 1896.
46. JOHN GALBRAITH, 1897.

VICE PRESIDENTS OF SECTIONS, CONTINUED.

Section D.—Mechanical Science and Engineering, continued.

47. JOHN GALBRAITH, 1898, in absence of M. E. COOLEY.
48. STORM BULL, 1899.
49. JOHN A. BRASHEAR, 1900.

Section E.—Geology and Geography.

31. E. T. COX, 1882.
32. C. H. HITCHCOCK, 1883.
33. N. H. WINCHELL, 1884.
34. EDWARD ORTON,* 1885.
35. T. C. CHAMBERLIN, 1886.
36. G. K. GILBERT, 1887.
37. GEORGE H. COOK,* 1888.
38. CHARLES A. WHITE, 1889.
39. JOHN C. BRANNER, 1890.
40. J. J. STEVENSON, 1891.
41. H. S. WILLIAMS, 1892.
42. CHARLES D. WALCOTT, 1893.
43. SAMUEL CALVIN, 1894.
44. JED. HOTCHKISS, 1895.
45. B. K. EMERSON, 1896.
46. { I. C. WHITE, 1897.
     { E. W. CLAYPOLE, 1897.
47. H. L. FAIRCHILD, 1898.
48. J. F. WHITEAVES, 1899.
49. J. F. KEMP, 1900.

Section F.—Biology, 1882-92.

31. W. H. DALL, 1882.
32. W. J. BEAL, 1883.
33. E. D. COPE,* 1884.
34. T. J. BURRILL, 1885, in the absence of B. G. WILDER.
35. H. P. BOWDITCH, 1886.
36. W. G. FARLOW, 1887.
37. C. V. RILEY,* 1888.
38. GEORGE L. GOODALE, 1889.
39. C. S. MINOT, 1890.
40. J. M. COULTER, 1891.
41. S. H. GAGE, 1892.

Section F.—Zoology.

42. HENRY F. OSBORN, 1893.
43. J. A. LINTNER,* 1894, in place of S. H. SCUDDER, resigned.

44. L. O. HOWARD, 1895, in place of D. S. JORDAN, resigned.
45. THEO. GILL, 1896.
46. L. O. HOWARD, 1897, in place of G. BROWN GOODE,* deceased.
47. A. S. PACKARD, 1898.
48. S. H. GAGE, 1899.
49. C. B. DAVENPORT, 1900.

Section G.—Microscopy, 1882-85.

31. A. H. TUTTLE, 1882.
32. J. D. COX, 1883.
33. T. G. WORMLEY,* 1884.
34. S. H. GAGE, 1885.
    (Section united with F in 1886.)

Section G.—Botany.

42. CHARLES E. BESSEY, 1893.
43. { L. M. UNDERWOOD, 1894.
     { C. E. BESSEY, 1894.
44. J. C. ARTHUR, 1895.
45. N. L. BRITTON, 1896.
46. G. F. ATKINSON, 1897.
47. W. G. FARLOW, 1898.
48. C. R. BARNES, 1899.
49. W. TRELEASE, 1900.

Section H.—Anthropology.

31. ALEXANDER WINCHELL,*1882.
32. OTIS T. MASON, 1883.
33. EDWARD S. MORSE, 1884.
34. J. OWEN DORSEY,* 1885, in absence of W. H. DALL.
35. HORATIO HALE,* 1886.
36. D. G. BRINTON,* 1887.
37. CHARLES C. ABBOTT, 1888.
38. GARRICK MALLERY,* 1889.
39. FRANK BAKER, 1890.
40. JOSEPH JASTROW, 1891.
41. W. H. HOLMES, 1892.
42. J. OWEN DORSEY,* 1893.
43. FRANZ BOAS, 1894.
44. F. H. CUSHING, 1895.
45. ALICE C. FLETCHER, 1896.
46. W J McGEE, 1897.
47. J. McK. CATTELL, 1898.
48. THOMAS WILSON, 1899.
49. A. W. BUTLER, 1900.

VICE PRESIDENTS OF SECTIONS, CONTINUED.

Section 1.—Social and Economic Science.

31. E. B. ELLIOTT,* 1882.
32. FRANKLIN B. HOUGH,* 1883.
33. JOHN EATON,* 1884.
34. EDWARD ATKINSON, 1885.
35. JOSEPH CUMMINGS,* 1886.
36. H. E. ALVORD, 1887.
37. CHARLES W. SMILEY, 1888.
38. CHARLES S. HILL, 1889.
39. J. RICHARDS DODGE, 1890.

40. EDMUND J. JAMES, 1891.
41. L. F. WARD, 1892, in place of S. D. HORTON,* resigned.
42. WILLIAM H. BREWER, 1893.
43. HENRY FARQUHAR, 1894.
44. B. E. FERNOW, 1895.
45. W. L. LAZENBY, 1896.
46. R. T. COLBURN, 1897.
47. ARCHIBALD BLUE, 1898.
48. MARCUS BENJAMIN, 1899.
49. C. M. WOODWARD, 1900.

SECRETARIES.

General Secretaries, 1848–

1. WALTER R. JOHNSON,* 1848.
2. E. N. HORSFORD,* 1849, in absence of JEFFRIES WYMAN.*
3. L. R. GIBBS, 1850, in absence of E. C. HERRICK.*
4. E. C. HERRICK,* 1850.
5. WILLIAM B. ROGERS,* 1851, in absence of E. C. HERRICK.*
6. WILLIAM B. ROGERS,* 1851.
7. S. ST. JOHN,* 1853, in absence of J. D. DANA.*
8. J. LAWRENCE SMITH,* 1854.
9. WOLCOTT GIBBS, 1855.
10. B. A. GOULD,* 1856.
11. JOHN LECONTE,* 1857.
12. W. M. GILLESPIE,* 1858, in absence of WM. CHAUVENET.*
13. WILLIAM CHAUVENET,* 1859.
14. JOSEPH LECONTE, 1860.
15. ELIAS LOOMIS,* 1866, in the absence of W. P. TROWBRIDGE.*
16. C. S. LYMAN,* 1867.
17. SIMON NEWCOMB, 1868, in absence of A. P. ROCKWELL.
18. O. C. MARSH,* 1869.
19. F. W. PUTNAM, 1870, in absence of C. F. HARTT.*
20. F. W. PUTNAM, 1871.
21. EDWARD S. MORSE, 1872.
22. C. A. WHITE, 1873.

23. A. C. HAMLIN, 1874.
24. S. H. SCUDDER, 1875.
25. T. C. MENDENHALL, 1876.
26. AUG. R. GROTE, 1877.
27. H. C. BOLTON, 1878.
28. H. C. BOLTON, 1879, in the absence of GEORGE LITTLE.
29. J. K. REES, 1880.
30. C. V. RILEY,* 1881.
31. WILLIAM SAUNDERS, 1882.
32. J. R. EASTMAN, 1883.
33. ALFRED SPRINGER, 1884.
34. C. S. MINOT, 1885.
35. S. G. WILLIAMS, 1886.
36. WILLIAM H. PETTEE, 1887.
37. JULIUS POHLMAN, 1888.
38. C. LEO MEES, 1889.
39. H. C. BOLTON, 1890.
40. H. W. WILEY, 1891.
41. A. W. BUTLER, 1892.
42. T. H. NORTON, 1893.
43. H. L. FAIRCHILD, 1894.
44. JAS. LEWIS HOWE, 1895.
45. CHARLES R. BARNES, 1896.
46. ASAPH HALL, JR., 1897.
47. J. MCMAHON, 1898, in place of D. S. KELLICOTT,* deceased.
48. F. BEDELL, 1899.
49. CHARLES BASKERVILLE, 1900.

Permanent Secretaries, 1851–

5-7.   SPENCER F. BAIRD,* 1851-4.
8-17. JOSEPH LOVERING,* 1854-68.

SECRETARIES, CONTINUED.

18. F. W. PUTNAM, 1869, in the absence of J. LOVERING.*
19-21. JOSEPH LOVERING,* 1870-73.
22-46. F. W. PUTNAM, 1873-98.
47-53. L. O. HOWARD, 1898-1905.

Assistant General Secretaries, 1882-1887.

31. J. R. EASTMAN, 1882.
32. ALFRED SPRINGER, 1883.
33. C. S. MINOT, 1884, in absence of E. S. HOLDEN.
34. S. G. WILLIAMS, 1885, in absence of C. C. ABBOTT.
35. W. H. PETTEE, 1886.
36. J. C. ARTHUR, 1887.

Secretaries of the Council, 1888-

37. C. LEO MEES, 1888.
38. H. C. BOLTON, 1889.
39. H. W. WILEY, 1890.
40. A. W. BUTLER, 1891.
41. T. H. NORTON, 1892.
42. H. LEROY FAIRCHILD, 1893.
43. JAS. LEWIS HOWE, 1894.
44. CHARLES R. BARNES, 1895.
45. ASAPH HALL, JR., 1896.

46. D. S. KELLICOTT,* 1897.
47. FREDERICK BEDELL, 1898.
48. CHARLES BASKERVILLE, 1899.
49. WILLIAM HALLOCK, 1900.

Secretaries of Section A.—Mathematics, Physics, and Chemistry, 1875-81.

24. { S. P. LANGLEY, 1875.
     { T. C. MENDENHALL, 1875.
25. A. W. WRIGHT, 1876.
26. H. C. BOLTON, 1877.
27. F. E. NIPHER, 1878.
28. J. K. REES, 1879.
29. H. B. MASON, 1880.
30. E. T. TAPPAN, 1881, in the absence of JOHN TROWBRIDGE.

Secretaries of Section B.—Natural History, 1874-81.

24. EDWARD S. MORSE, 1875.
25. ALBERT H. TUTTLE, 1876.
26. WILLIAM H. DALL, 1877.
27. GEORGE LITTLE, 1878.
28. WILLIAM H. DALL, 1879, in the absence of A. C. WETHERBY.
29. CHARLES V. RILEY,* 1880.
30. WILLIAM SAUNDERS, 1881.

SECRETARIES OF SUBSECTIONS, 1875-81.

Subsection of Chemistry.

24. F. W. CLARKE, 1875.
25. H. C. BOLTON, 1876.
26. P. SCHWEITZER, 1877.
27. A. P. S. STUART, 1878.
28. W. R. NICHOLS,* 1879.
29. C. E. MUNROE, 1880.
30. ALFRED SPRINGER, 1881, in absence of R. B. WARDER.

Subsection of Entomology.

30. B. P. MANN, 1881.

Subsection of Anthropology.

24. F. W. PUTNAM, 1875.
25. OTIS T. MASON, 1876.
26, 27. United with Section B.
28, 29, 30. J. G. HENDERSON, 1879-81.

Subsection of Microscopy.

25. E. W. MORLEY, 1876.
26. T. O. SOMMERS, JR., 1877.
27. G. J. ENGELMANN, 1878.
28, 29. A. B. HERVEY, 1879-80.
30. W. H. SEAMAN, 1881, in the absence of S. P. SHARPLES.

SECRETARIES OF THE SECTIONS, 1882-

Section A.—Mathematics and Astronomy.

31. H. T. EDDY, 1882.
32. G. W. HOUGH, 1883, in the absence of W. W. JOHNSON.
33. G. W. HOUGH, 1884.
34. E. W. HYDE, 1885.
35. S. C. CHANDLER, 1886.
36. H. M. PAUL, 1887.
37. C. C. DOOLITTLE, 1888.

SECRETARIES OF SECTIONS, CONTINUED.

Section A.—Mathematics and Astronomy, continued.

38. G. C. COMSTOCK, 1889.
39. W. W. BEMAN, 1890.
40. F. H. BIGELOW, 1891.
41. WINSLOW UPTON, 1892.
42. C. A. WALDO, 1893, in the absence of A. W. PHILLIPS.
43. J. C. KERSHNER, 1894, in place of W. W. BEMAN, resigned.
44. ASAPH HALL, JR., 1895, in place of E. H. MOORE, resigned.
45. EDWIN B. FROST, 1896.
46. JAMES MCMAHON, 1897.
47. WINSLOW UPTON, 1898, in place of ALEXANDER ZIWET, resigned.
48. JOHN F. HAYFORD, 1899.
49. W. M. STRONG, 1900.

Section B.—Physics.

31. C. S. HASTINGS, 1882.
32. F. E. NIPHER, 1883, in the absence of C. K. WEAD.
33. N. D. C. HODGES, 1884.
34. B. F. THOMAS, 1885, in place of A. A. MICHELSON, resigned.
35. H. S. CARHART, 1886.
36. C. LEO MEES, 1887.
37. ALEX. MACFARLANE, 1888.
38. E. L. NICHOLS, 1889.
39. E. M. AVERY, 1890.
40. ALEX.MACFARLANE, 1891.
41. BROWN AYRES, 1892.
42. W. LECONTE STEVENS, 1893.
43. B. W. SNOW, 1894.
44. E. MERRITT, 1895.
45. FRANK P. WHITMAN, 1896.
46. FREDERICK BEDELL, 1897.
47. W. S. FRANKLIN, 1898, in place of E. B. ROSA, resigned.
48. WILLIAM HALLOCK, 1899.
49. R. A. FESSENDEN, 1900.

Section C.—Chemistry.

31. ALFRED SPRINGER, 1882.
32. { J. W. LANGLEY, 1883.
  { W. MCMURTRIE, 1883.

33. H. CARMICHAEL, 1884, in the absence of R. B. WARDER.
34. F. P. DUNNINGTON, 1885.
35. W. MCMURTRIE, 1886.
36. C. F. MABERY, 1887.
37. W. L. DUDLEY, 1888.
38. EDWARD HART, 1889.
39. W. A. NOYES, 1890.
40. T. H. NORTON, 1891.
41. JAS. LEWIS HOWE, 1892.
42. H. N. STOKES, 1893, in the absence of J. U. NEF.
43. MORRIS LOEB, 1894, in place of S. M. BABCOCK, resigned.
44. { W. P. MASON, 1895.
  { W. O. ATWATER, 1895.
45. FRANK P. VENABLE, 1896.
46. P. C. FREER, 1897.
47. C. BASKERVILLE, 1898.
48. H. A. WEBER, 1899.
49. A. A. NOYES, 1900.

Section D.—Mechanical Science and Engineering.

31. J. BURKITT WEBB, 1882, in the absence of C. B. DUDLEY.
32. J. BURKITT WEBB, 1883, *pro tempore.*
33. J. BURKITT WEBB, 1884.
34. C. J. H. WOODBURY, 1885.
35. WILLIAM KENT, 1886.
36. G. M. BOND, 1887.
37. ARTHUR BEARDSLEY, 1888.
38. W. B. WARNER, 1889.
39. THOMAS GRAY, 1890.
40. WILLIAM KENT, 1891.
41. O. H. LANDRETH, 1892.
42. D. S. JACOBUS, 1893.
43. JOHN H. KINEALY, 1894.
44. H. S. JACOBY, 1895.
45. JOHN GALBRAITH, 1896.
46. JOHN J. FLATHER, 1897.
47. W. S. ALDRICH, 1898.
48. J. M. PORTER, 1899.
49. W. T. MAGRUDER, 1900.

SECRETARIES OF THE SECTIONS, CONTINUED.

Section E.—Geology and Geography.

31. H. S. WILLIAMS, 1882, in the absence of C. E. DUTTON.
32. A. A. JULIEN, 1883.
33. E. A. SMITH, 1884.
34. G. K. GILBERT, 1885, in the absence of H. C. LEWIS.*
35. E. W. CLAYPOLE, 1886.
36. W. M. DAVIS, 1887, in the absence of T. B. COMSTOCK.
37. JOHN C. BRANNER, 1888.
38. JOHN C. BRANNER, 1889.
39. SAMUEL CALVIN, 1890.
40. W J McGEE, 1891.
41. R. D. SALISBURY, 1892.
42. W. H. HOBBS,* 1893, in place of R. T. HILL, resigned.
43. JED. HOTCHKISS,* 1894, in place of W. M. DAVIS, resigned.
44. J. PERRIN SMITH, 1895.
45. W. N. RICE, 1896, in place of A. C. GILL, resigned.
46. C. H. SMITH, JR., 1897.
47. WARREN UPHAM, 1898.
48. ARTHUR HOLLICK, 1899.
49. J. A. HOLMES, 1900.

Section F.—Biology, 1882-92.

31. WILLIAM OSLER, 1882, in the absence of C. S. MINOT.
32. S. A. FORBES, 1883.
33. C. E. BESSEY, 1884.
34. J. A. LINTNER,* 1885, in place of C. H. FERNALD, resigned.
35. J. C. ARTHUR, 1886.
36. J. H. COMSTOCK, 1887.
37. B. E. FERNOW, 1888.
38. A. W. BUTLER, 1889.
39. J. M. COULTER, 1890.
40. A. J. COOK, 1891.
41. D. B. HALSTEAD, 1892.

Section F.—Zoology.

42. L. O. HOWARD, 1893.

43. JOHN B. SMITH, 1894, in place of WM. LIBBY, JR., resigned.
44. C. W. HARGITT, 1895, in place of S. A. FORBES, resigned.
45. D. S. KELLICOTT,* 1896.
46. C. C. NUTTING, 1897.
47. R. T. JACKSON, 1898, in place of C. W. STILES, resigned.
48. C. L. MARLATT, 1899, in place of F. W. TRUE, resigned.
49. C. H. EIGENMANN, 1900.

Section G.—Microscopy, 1882-85.

31. ROBERT BROWN, JR., 1882.
32. CARL SEILER, 1883.
33. ROMYN HITCHCOCK, 1884.
34. W. H. WALMSLEY, 1885.

Section G.—Botany.

42. B. T. GALLOWAY, 1893, in the absence of F. V. COVILLE.
43. CHARLES R. BARNES, 1894.
44. { B. T. GALLOWAY, 1895.
     { M. B. WAITE, 1895.
45. GEORGE F. ATKINSON, 1896.
46. F. C. NEWCOMBE, 1897.
47. ERWIN F. SMITH, 1898.
48. W. A. KELLERMAN, 1899.
49. D. T. MacDOUGAL, 1900.

Section H.—Anthropology.

31. OTIS T. MASON, 1882.
32. G. H. PERKINS, 1883.
33. G. H. PERKINS, 1884, in the absence of W. H. HOLMES.
34. ERMINNIE A. SMITH,* 1885.
35. A. W. BUTLER, 1886.
36. CHARLES C. ABBOTT, 1887, in absence of F. W. LANGDON.
37. FRANK BAKER, 1888.
38. W. M. BEAUCHAMP, 1889.
39. JOSEPH JASTROW, 1890.
40. W. H. HOLMES, 1891.
41. W. M. BEAUCHAMP, 1892, in place of S. CULIN, resigned.
42. WARREN K. MOOREHEAD, 1893.
43. A. F. CHAMBERLIN, 1894.

SECRETARIES OF THE SECTIONS, CONTINUED.

Section H.—Anthropology, continued.

44. { STEWART CULIN and W. W. TOOKER, 1895, in place of ANITA N. McGEE, resigned.
45. G. H. PERKINS, 1896, in place of J. G. BOURKE,* deceased.
46. ANITA N. McGEE, 1897, in place of HARLAN I. SMITH, resigned.
47. MARSHALL H. SAVILLE, 1898.
48. E. W. SCRIPTURE, 1899, in place of GEORGE A. DORSEY, resigned.
49. FRANK RUSSELL, 1900.

Section I.—Social and Economic Science.

31. { FRANKLIN B. HOUGH,* 1882.
J. RICHARDS DODGE, 1882.

32. JOSEPH CUMMINGS,* 1883.
33. CHARLES W. SMILEY, 1884.
34. CHARLES W. SMILEY, 1885, in absence of J. W. CHICKERING.
35. H. E. ALVORD, 1886.
36. W. R. LAZENBY, 1887.
37. CHARLES S. HILL, 1888.
38. J. RICHARDS DODGE, 1889.
39. B. E. FERNOW, 1890.
40. B. E. FERNOW, 1891.
41. HENRY FARQUHAR, 1892, in place of L. F. WARD, made Vice President.
42. NELLIE S. KEDZIE, 1893.
43. MANLEY MILES, 1894.
44. W. R. LAZENBY, 1895, in place of E. A. ROSS, resigned.
45. R. T. COLBURN, 1896.
46. ARCHIBALD BLUE, 1897.
47. MARCUS BENJAMIN, 1898.
48. CALVIN M. WOODWARD, 1899.
49. H. T. NEWCOMB, 1900.

TREASURERS.

1. JEFFRIES WYMAN,* 1848.
2. A. L. ELWYN,* 1849.
3. ST. J. RAVENEL,* 1850, in the absence of A. L. ELWYN.*
4. A. L. ELWYN,* 1850.
5. SPENCER F. BAIRD,* 1851, in absence of A. L. ELWYN.*
6-7. A. L. ELWYN,* 1851-53.

8. J. L. LeCONTE,* 1854, in absence of A. L. ELWYN.*
9-19. A. L. ELWYN,* 1855-1870.
20-30. WILLIAM S. VAUX,* 1871-1881.
32-42. WILLIAM LILLY,* 1882-93.
43-49. R. S. WOODWARD, 1894-1900.

Commonwealth of Massachusetts.

In the Year one Thousand Eight Hundred and Seventy-Four.

AN ACT

TO INCORPORATE THE "AMERICAN ASSOCIATION FOR THE ADVANCE-
MENT OF SCIENCE."

*Be it enacted by the Senate and House of Representatives, in General
Court assembled, and by the authority of the same, as follows :*

SECTION 1. Joseph Henry of Washington, Benjamin Pierce of Cam-
bridge, James D. Dana of New Haven, James Hall of Albany, Alexis
Caswell of Providence, Stephen Alexander of Princeton, Isaac Lea of
Philadelphia, F. A. P. Barnard of New York, John S. Newberry of Cleve-
land, B. A. Gould of Cambridge, T. Sterry Hunt of Boston, Asa Gray of
Cambridge, J. Lawrence Smith of Louisville, Joseph Lovering of Cam-
bridge, and John LeConte of Philadelphia, their associates, the officers
and members of the Association, known as the "American Association
for the Advancement of Science," and their successors, are hereby made
a corporation by the name of the "American Association for the Ad-
vancement of Science," for the purpose of receiving, purchasing, hold-
ing, and conveying real and personal property, which it now is, or here-
after may be, possessed of, with all the powers and privileges, and subject
to the restrictions, duties and liabilities set forth in the general laws
which now or hereafter may be in force and applicable to such corpora-
tions.

SECTION 2. Said corporation may have and hold by purchase, grant,
gift, or otherwise, real estate not exceeding one hundred thousand dollars
in value, and personal estate of the value of two hundred and fifty
thousand dollars.

SECTION 3. Any two of the corporators above named are hereby author-
ized to call the first meeting of the said corporation in the month of
August next ensuing, by notice thereof "by mail," to each member of
the said Association.

SECTION 4. This act shall take effect upon its passage.

HOUSE OF REPRESENTATIVES, March 10, 1874.
Passed to be enacted,
JOHN E. SANFORD, *Speaker.*

IN SENATE, March 17, 1874.
Passed to be enacted.   March 19, 1874.
GEO. B. LORING, *President.*   Approved,
W. B. WASHBURN.

SECRETARY'S DEPARTMENT,
Boston, April 3, 1874.
A true copy, Attest :
DAVID PULSIFER,
Deputy Secretary of the Commonwealth.

(25)

CONSTITUTION

OF THE

AMERICAN ASSOCIATION FOR THE ADVANCE-MENT OF SCIENCE.

Incorporated by Act of the General Court of the Commonwealth of Massachusetts.

OBJECTS.

ARTICLE I.   The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of America, to give a stronger and more general impulse and more systematic direction to scientific research, and to procure for the labors of scientific men increased facilities and a wider usefulness.

MEMBERS, FELLOWS, PATRONS, AND HONORARY FELLOWS.

ART. 2.   The Association shall consist of Members, Fellows, Patrons, Corresponding Members and Honorary Fellows.

ART. 3.   Any person may become a Member of the Association upon recommendation in writing by two members or fellows, and election by the Council.   Any incorporated scientific society or institution, or any public or incorporated library, may be enrolled as a member of the Association by vote of the Council by payment of the initiation fee; such society, institution or library may be represented by either the President, Curator, Director, or Librarian presenting proper credentials at any meeting of the Association for which the assessment has been paid.

Associates for any single meeting shall be admitted on the payment of three dollars ; such Associates to have all the privileges of the meeting, except reading papers and voting.

Members of scientific societies whose meetings are contemporaneous with or immediately subsequent to that of the Association and which are recognized by vote of the Council as 'Affiliated Societies,' may become Associate Members for that meeting on the payment of three dollars. They shall be entitled to all the privileges of membership except voting or appointment to office, but their names shall not appear in the list of members printed in the annual report.

Any Member or Fellow of any national scientific or educational institution, or of any society or academy of science, or any country not in America, who may be present at any meeting of the Association shall, on presenting the proper credentials, be enrolled without fee as a Foreign Associate, and shall be entitled to all the privileges of the meeting except voting on matters of business.

ART. 4. Fellows shall be elected by the Council from such of the members as are professionally engaged in science, or have, by their labors, aided in advancing science. The election of fellows shall be by ballot and a majority vote of the members of the Council at a designated meeting of the Council.

ART. 5. Any person paying to the Association the sum of one thousand dollars shall be classed as a Patron, and shall be entitled to all the privileges of a member and to all its publications.

ART. 6. Honorary Fellows of the Association, not exceeding three for each section, may be elected; the nominations to be made by the Council and approved by ballot in the respective sections before election by ballot in General Session. Honorary Fellows shall be entitled to all the privileges of Fellows and shall be exempt from all fees and assessments, and entitled to all publications of the Association issued after the date of their election. Corresponding Members shall consist of such scientists not residing in America as may be elected by the Council, and their number shall be limited to fifty. Corresponding Members shall be entitled to all the privileges of members and to the annual volumes of Proceedings published subsequent to their election.

ART. 7. The name of any member or fellow two years in arrears for annual dues shall be erased from the list of the Association, provided that two notices of indebtedness, at an interval of at least three months, shall have been given; and no such person shall be restored until he has paid his arrearages or has been reelected. The Council shall have power to exclude from the Association any member or fellow, on satisfactory evidence that said member or fellow is an improper person to be connected with the Association, or has in the estimation of the Council made improper use of his membership or fellowship.

ART. 8. No member or fellow shall take part in the organization of, or hold office in, more than one section at any one meeting.

OFFICERS.

ART. 9. The officers of the Association shall be elected by ballot by

the General Committee from the fellows, and shall consist of a President, a Vice President from each section, a Permanent Secretary, a General Secretary, a Secretary of the Council, a Treasurer, and a Secretary of each Section; these, with the exception of the Permanent Secretary, shall be elected at each meeting for the following one and, with the exception of the Treasurer and the Permanent Secretary, shall not be reeligible for the next two meetings. The term of office of Permanent Secretary shall be five years.

ART. 10. The President, or, in his absence, the senior Vice President present, shall preside at all General Sessions of the Association and at all meetings of the Council. It shall also be the duty of the President to give an address at a General Session of the Association at the meeting following that over which he presided.

ART. 11. The Vice Presidents shall be chairmen of their respective Sections, and of their Sectional Committees, and it shall be part of their duty to give an address, each before his own section, at such time as the council shall determine at the meeting subsequent to that at which he presides. The Vice Presidents may appoint temporary Chairmen to preside over the sessions of their sections, but shall not delegate their other duties. The Vice Presidents shall have seniority in order of their continuous membership in the Association.

ART. 12. The General Secretary shall be the Secretary of all General Sessions of the Association, and shall keep a record of the business of these sessions. He shall receive the records from the Secretaries of the Sections, which, after examination, he shall transmit with his own records to the Permanent Secretary within two weeks after the adjournment of the meeting.

ART. 13. The Secretary of the Council shall keep the records of the Council. He shall give to the Secretary of each Section the titles of papers assigned to it by the Council. He shall receive proposals for membership and bring them before the Council.

ART. 14. The Permanent Secretary shall be the executive officer of the Association under the direction of the Council. He shall attend to all business not specially referred to committees nor otherwise constitutionally provided for. He shall keep an account of all business that he has transacted for the Association, and make annually a general report for publication in the annual volume of Proceedings. He shall attend to the printing and distribution of the annual volume of Proceedings,

and all other printing ordered by the Association. He shall issue a circular of information to members and fellows at least three months before each meeting, and shall, in connection with the Local Committee, make all necessary arrangements for the meetings of the Association. He shall provide the Secretaries of the Association with such books and stationery as may be required for their records and business, and shall provide members and fellows with such blank forms as may be required for facilitating the business of the Association. He shall collect all assessments and admission fees, and notify members and fellows of their election, and of any arrearages. He shall receive, and bring before the Council, the titles and abstracts of papers proposed to be read before the Association. He shall keep an account of all receipts and expenditures of the Association, and report the same annually at the first meeting of the Council, and shall pay over to the Treasurer such unexpended funds as the Council may direct. He shall receive and hold in trust for the Association all books, pamphlets, and manuscripts belonging to the Association, and allow the use of the same under the provisions of the Constitution and the orders of the Council. He shall receive all communications addressed to the Association during the intervals between meetings, and properly attend to the same. He shall at each meeting report the names of fellows and members who have died since the preceding meeting. He shall be allowed a salary which shall be determined by the Council, and may employ one or more clerks at such compensation as may be agreed upon by the Council.

ART. 15. The Treasurer shall invest the funds received by him in such securities as may be directed by the Council. He shall annually present to the Council an account of the funds in his charge. No expenditure of the principal in the hands of the Treasurer shall be made without a unanimous vote of the Council, and no expenditure of the income received by the Treasurer shall be made without a two-thirds' vote of the Council. The Treasurer shall give bonds for the faithful performance of his duty in such manner and sum as the Council shall from time to time direct.

ART. 16. The Secretaries of the Sections shall keep the records of their respective sections, and, at the close of the meeting, give the same, including the records of subsections, to the General Secretary. They shall also be the Secretaries of the Sectional Committees. The Secretaries shall have seniority in order of their continuous membership in the Association.

(29)

ART. 17. In case of a vacancy in the office of President, the Senior Vice President shall preside, as provided in Article 10, until the General Committee can be assembled and the vacancy filled by election. Vacancies in the offices of Vice President, Permanent Secretary, Secretary of the Council, Secretaries of the Sections, and Treasurer, shall be filled by the Council by ballot.

ART. 18. The Council shall consist of the past Presidents, and the Vice Presidents of the last two meetings, together with the President, the Vice Presidents, the Permanent Secretary, the General Secretary, the Secretary of the Council, the Secretaries of the Sections, and the Treasurer of the current meeting, with the addition of one fellow elected from each Section by ballot on the first day of its meeting. The members present at any regularly called meeting of the Council, provided there are at least five, shall form a quorum for the transaction of business. The Council shall meet on the day preceding each annual meeting of the Association, and arrange the program for the first day of the sessions. The time and place of this first meeting shall be designated by the Permanent Secretary. Unless otherwise agreed upon, regular meetings of the Council shall be held in the Council room at 9 o'clock, A.M., on each day of the meeting of the Association. Special meetings of the Council may be called at any time by the President. The Council shall be the board of supervision of the Association, and no business shall be transacted by the Association that has not first been referred to, or originated with, the Council. The Council shall decide which papers, discussions, and other proceedings shall be published, and have the general direction of the publications of the Association ; manage the financial affairs of the Association ; arrange the business and programs for General Sessions ; suggest subjects for discussion, investigation or reports ; elect members and fellows ; and receive and act upon all invitations extended to the Association and report the same at a General Session of the Association. The Council shall receive all reports of Special Committees and decide upon them, and only such shall be read in General Session as the Council shall direct. The Council shall appoint at each meeting the following subcommittees who shall act, subject to appeal to the whole Council, until their successors are appointed at the following meeting: 1, on Papers and Reports ; 2, on Members ; 3, on Fellows.

ART. 19. The General Committee shall consist of the Council and one Member or Fellow elected by each of the Sections, who shall serve until

their successors are elected. It shall be the duty of the Committee to meet at the call of the President and elect the general officers for the following meeting of the Association. It shall also be the duty of this Committee to fix the time and place for the next meeting. The Vice President and Secretary of each Section shall be recommended to the General Committee by the Sectional Committee.

MEETINGS.

ART. 20. The Association shall hold a public meeting annually, for one week or longer, at such time and place as may be determined by vote of the General Committee, and the preliminary arrangements for each meeting shall be made by the Local Committee, in conjunction with the Permanent Secretary and such other persons as the Council may designate.

ART. 21. A General Session shall be held at 10 o'clock, A. M., on the first day of the meeting, and at such other times as the Council may direct.

SECTIONS AND SUBSECTIONS.

ART. 22. The Association shall be divided into Sections, namely:—A, *Mathematics and Astronomy;* B, *Physics;* C, *Chemistry, including its application to Agriculture and the Arts;* D, *Mechanical Science and Engineering;* E, *Geology and Geography;* F, *Zoology;* G, *Botany;* H, *Anthropology;* I, *Social and Economic Science.* The Council shall have power to consolidate any two or more Sections temporarily, and such consolidated Sections shall be presided over by the senior Vice President and Secretary of the Sections comprising it.

ART. 23. Immediately on the organization of a Section there shall be three members or fellows elected by ballot after open nomination, who, with the Vice President and Secretary and the Vice President and Secretary of the preceding meeting, shall form its Sectional Committee. The Sectional Committees shall have power to fill vacancies in their own numbers. Meetings of the Sections shall not be held at the same time with a General Session. The Sectional Committee may invite distinguished Foreign Associates present at any meeting to serve as Honorary members of said Committee.

ART. 24. The Sectional Committee of any Section may at its pleasure form one or more temporary Subsections, and may designate the officers thereof. The Secretary of a Subsection shall, at the close of the meeting, transmit his records to the Secretary of the Section.

ART. 25. No paper shall be read in any Section or Subsection until it has been placed on the program of the day by the Sectional Committee.

SECTIONAL COMMITTEES.

ART. 26. The Sectional Committees shall arrange and direct the business of their respective Sections. They shall prepare the daily programs and give them to the Permanent Secretary for printing at the earliest moment practicable. No titles of papers shall be entered on the daily programs except such as have passed the Committee. No change shall be made in the program for the day in a Section without the consent of the Sectional Committee. The Sectional Committees may refuse to place the title of any paper on the program; but every such title, with the abstract of the paper or the paper itself, must be referred to the Council with the reasons why it was refused. The Sectional Committee shall also make nominations to the General Committee for Vice President and Secretary of their respective Sections as provided for in Article 19.

ART. 27. The Sectional Committees shall examine all papers and abstracts referred to the Sections, and they shall not place on the program any paper inconsistent with the character of the Association; and to this end they have power to call for any paper, the character of which may not be sufficiently understood from the abstract submitted.

PAPERS AND COMMUNICATIONS.

ART. 28. All members and fellows must forward to the Secretary of the proper Section or to the Permanent Secretary, as early as possible, and when practicable before the convening of the Association, full titles of all the papers which they propose to present during the meeting, with a statement of the time that each will occupy in delivery, and also such abstracts of their contents as will give a general idea of their nature; and no title shall be considered by a Sectional Committee until an abstract of the paper or the paper itself has been received.

ART. 29. If the author of any paper be not ready when called upon in the regular order to the official program, the title may be dropped to the bottom of the list.

ART. 30. Whenever practicable, the proceedings and discussions at General Sessions, Sections and Subsections shall be reported by professional reporters but such reports shall not appear in print as the official reports of the Association unless revised by the Secretaries.

PRINTED PROCEEDINGS.

ART. 31. The Permanent Secretary shall have the Proceedings of each meeting printed in an octavo volume as soon after the meeting as

possible, beginning one month after adjournment. Authors must prepare their papers or abstracts ready for the press, and these must be in the hands of the Secretaries of the Sections before the final adjournment of the meeting, otherwise only the titles will appear in the printed volume. The Council shall have power to order the printing of any paper by abstract or title only. · Whenever practicable, proofs shall be forwarded to authors for revision. If any additions or substantial alterations are made by the author of a paper after its submission to the Secretary, the same shall be distinctly indicated. Illustrations must be provided for by the authors of the papers, or by a special appropriation from the Council. Immediately on publication of the volume, a copy shall be forwarded to every member and fellow of the Association who shall have paid the assessment for the meeting to which it relates, and it shall also be offered for sale by the Permanent Secretary at such price as may be determined by the Council. The Council shall also designate the institutions to which copies shall be distributed.

LOCAL COMMITTEE.

ART. 32. The Local Committee shall consist of persons interested in the objects of the Association and residing at or near the place of the proposed meeting. It is expected that the Local Committee, assisted by the officers of the Association, will make all essential arrangements for the meeting, and issue a circular giving necessary particulars, at least one month before the meeting.

LIBRARY OF THE ASSOCIATION.

ART. 33. All books and pamphlets received by the Association shall be in the charge of the Permanent Secretary, who shall have a list of the same printed and shall furnish a copy to any member or fellow on application. Members and fellows who have paid their assessments in full shall be allowed to call for books and pamphlets, which shall be delivered to them at their expense, on their giving a receipt agreeing to make good any loss or damage and to return the same free of expense to the Secretary at the time specified in the receipt given.* All books and pamphlets in circulation must be returned at each meeting. Not more than five books, including volumes, parts of volumes, and pamphlets, shall be held at one time by any member or fellow. Any book may be withheld from circulation by order of the Council. [The Library of the Association was, by vote of the Council in 1895, placed on deposit in the Library of the University of Cincinnati, Ohio. Members can obtain the use of

books by writing to the Librarian of the University Library, Cincinnati, Ohio.]

ADMISSION FEE AND ASSESSMENTS.

ART. 34. The admission fee for members shall be five dollars in addition to the annual assessment. On the election of any member as a fellow an additional fee of two dollars shall be paid.   /

ART. 35. The annual assessment for members and fellows shall be three dollars.

ART. 36. Any member or fellow who shall pay the sum of fifty dollars to the Association, at any one time, shall become a Life Member, and as such, shall be exempt from all further assessments, and shall be entitled to the Proceedings of the Association. All money thus received shall be invested as a permanent fund, the income of which, during the life of the member, shall form a part of the general fund of the Association; but, after his death, shall be used only to assist in original research, unless otherwise directed by unanimous vote of the Council.

ART. 37. All fees and assessments must be paid to the Permanent Secretary, who shall give proper receipts for the same.

ACCOUNTS.

ART. 38. The accounts of the Permanent Secretary and of the Treasurer shall be audited annually, by Auditors appointed by the Council.

ALTERATIONS OF THE CONSTITUTION.

ART. 39. No part of this Constitution shall be amended or annulled, without the concurrence of three-fourths of the members and fellows present in General Session, after notice given at a General Session of a preceding meeting of the Association.

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.[1]

PATRONS.[2]

THOMPSON, MRS. ELIZABETH, Stamford, Conn. (22). (Died July, 1899.
LILLY, GEN. WILLIAM, Mauch Chunk, Pa. (28). (Died Dec. 1, 1893.)
HERRMAN, MRS. ESTHER, 59 West 56th St., New York, N. Y. (29).
McMILLIN, EMERSON, 40 Wall St., New York, N. Y. (37).

MEMBERS.[3]

Abraham, Abraham, Brooklyn, N. Y. (43).
Abraham, Walter D., 1833 Venango St., Tioga, Philadelphia, Pa. (47).
Adams, Comfort A., Harvard University, Cambridge, Mass. (47).
Adams, C. E., M.D., 29 West Broadway, Bangor, Me. (43). **F**
Aldis, Owen F., 230 Monadnock Block, Chicago, Ill. (41). **H**
Alford, William V., Garrettsville, Ohio (48). **E H**
Allderdice, Wm. H., Lieut. U. S. Navy, care Navy Department, Washington, D. C. (33). **D**
Allen, J. M., Hartford, Conn. (22). **D**

[1] The numbers in parentheses indicate the meeting at which the member was elected. The black letters at the end of line indicate the sections to which members belong. The constitution requires that the names of all members two years in arrears shall be omitted from the list, but their names will be restored on payment of arrearages. Members not in arrears are entitled to the annual volume of Proceedings bound in paper. *The payment of ten dollars at one time entitles a member to the subsequent volumes to which he may be entitled, bound in cloth, or by the payment of twenty dollars, to such volumes bound in half morocco.*

[2] Persons contributing one thousand dollars or more to the Association are classed as Patrons, and are entitled to the privileges of members and to the publications.

The names of Patrons are to remain permanently on the list.

[3] Any Member or Fellow may become a Life Member by the payment of fifty dollars. The income of the money derived from a life membership is used for the general purposes of the Association during the life of the member; afterwards it is to be used to aid in original research. Life Members are exempt from the annual assessment, and are entitled to the annual volume. The names of Life Members are printed in small capitals in the regular list of Members and Fellows.

(35)

Allen, Walter S., 34 S. Sixth St., New Bedford, Mass. (39). C I
Alspach, E. F., 455 West Sixth Ave., Columbus, Ohio (48). H
Anderson, Frank P., Epworth, Iowa (46).
Andrews, E. R., Rochester, N. Y. (41).
Andrews, Wm. C., care Claflin Thayer & Co., P. O. Box 2000, New York,
     N. Y. (46).
Anthony, Mrs. Emilia C., Gouverneur, St. Lawrence Co., N. Y. (47). G
Appleton, Rev. Edw. W., D.D., Ashbourne, Montgomery Co., Pa. (28).
Ashe, W. W., State Geological Survey, Raleigh, N. C. (47).
Atkins, Prof. Martin D., Agricultural College, Mich. (48). B
Atkinson, Jno. B., Earlington, Hopkins Co., Ky. (26). D
Austin, Dr. George M., Wilmington, Ohio. (48) E F
AVERY, SAMUEL P., 4 E. 38th St., New York, N. Y. (36).
Ayer, Edward Everett, Room 12, The Rookery, Chicago, Ill. (37). H
Ayres, Horace B., Carlton, Minn. (40).
Baker, A. G., Springfield, Mass. (44).
Baker, O. M., 499 Main St., Springfield, Mass. (44).
Baker, Prof. Thos. R., Winter Park, Fla. (47).
Balch, Samuel W., 41 Wall St., New York, N. Y. (43).
Baldwin, Mrs. G. H., 3 Madison Ave., Detroit, Mich. (34). H
Baldwin, Herbert B., 906 Broad St., Newark, N. J. (43).
BALDWIN, S. PRENTISS, 736 Prospect St., Cleveland, Ohio. (47). E
Ball, Emma L., 207 West Eighth Ave., Columbus, Ohio (48).
Balliet, Thomas M., Supt. of Schools, Springfield, Mass. (48). I H
BANGS, LEMUEL BOLTON, M.D., 127 E. 34th St., New York, N. Y. (36).
Bangs, Outram, 22 Pemberton Square, Boston, Mass. (47). F
BARGE, B. F., Mauch Chunk, Pa. (33).
Barker, Mrs. Martha M., 26 Eleventh St., Lowell, Mass. (31). E H
Barker, Mrs. Mary E., Collinsville, Conn. (45).
Barnhart, Arthur M., 185 Monroe St., Chicago, Ill. (42).
Barrows, Franklin W., 45 Park St. Buffalo, N. Y. (47). F
Barton, George Hunt, 16 Lexington Ave., Cambridge, Mass. (47).
Bartow, Edward, Ph.D., Kansas State University, Lawrence, Kan. (47). C
Barwell, John William, Waukegan, Ill. (47).
Bashore, Dr. Harvey B., West Fairview, Pa. (46). E
Bates, George Williams, 32033 Buhl Block, Detroit, Mich. (46). H I
Bates, Miss Octavie Williams, A.B. LL.M., 53 Bagg St., Detroit, Mich.
     (47). I
Batterson, J. G., Hartford, Conn. (23).
Bausch, Henry, P. O. Drawer 1033, Rochester, N. Y. (41).
Baxter, James N., care H. E. and C. Baxter, cor. Division and Bedford Sts.,
     Brooklyn, N. Y. (36).
Beach, Spencer Ambrose, N. Y. Experiment Station, Geneva, N. Y. (41). G
Bean, Thos. E., Box 441, Galena, Ill. (28). F
Becher, Franklin A., 406 Irving Place, Milwaukee, Wis. (41). I A
Beckwith, Miss Florence, 394 Alexander St., Rochester, N. Y. (45). G
BELL, C. M., M.D., 320 Fifth Ave., New York, N. Y. (36).

Bennett, Rev. N. E., Wilmington, Ohio (47). **A**

Bennett, Prof. William Z., Univ. of Wooster, Wooster. Ohio (48). **C**

Benneville, James S. de, University Club, 1510 Walnut St., Philadelphia, Pa. (46). **C**

Bentley, Wilson A., Nashville, Vt. (48).

Bergey, David H., S. E. Cor. 34th and Locust Sts., Philadelphia, Pa. (48).

Berry, John Wilson, C.E., Pittston, Pa. (47). **A B D E I**

Bevier, Miss Isabel, Plymouth, Ohio (46). **C**

Beyer, Samuel W., Iowa Agricultural College, Ames, Ia. (47). **E**

Bickford, Miss Elizabeth E., Ph.D., Vassar College, Poughkeepsie, N. Y. (46). **F**

Biddle, James G., 944 Drexel Building, Philadelphia, Pa. (33).

Bien, Julius, 140 Sixth Ave., New York, N. Y. (34). **E H**

Biggar, Hamilton F., M.D., 176 Euclid Ave., Cleveland, Ohio (40). **B F**

BISHOP, HEBER R., Mills Building, New York, N. Y. (36).

Bitting, Prof. A. W., Purdue Univ., Lafayette, Ind. (46).

Blair, Mrs. Eliza N., Manchester, N. H. (40).

Blake, Edwin Mortimer, 230 Washington Ave., Brooklyn, N. Y. (43).

BLISH, W. G., Niles, Mich. (33). **B D**

BLOUNT, HENRY FITCH, "The Oaks," Washington, D. C. (32). **I B**

Bogert, Marston Taylor, Havemeyer Hall, Columbia University, New York, N. Y. (47). **C**

Boland, Miss Mary A., 117 West Newton St., Boston, Mass. (47).

Bonwill, W. G. A., D.D.S., 2009 Chestnut St., Philadelphia, Pa. (47). **F H**

Bookman, Samuel, Ph.D., 9 E. 62nd St., New York, N. Y. (47). **C**

Booraem, J. V. V., 204 Lincoln Place, Brooklyn, N. Y. (36).

Borner, William, The Majestic, 52 and 54 Walton Place, Chicago, Ill. (44).

Boutwell, John Mason, 10 Weld Hall, Cambridge, Mass. (46). **E**

Bowker, R. R., 28 Elm St., New York, N. Y. (43). **B**

Bownocker, Prof. J. A., Ohio State University, Columbus, O. (48). **E**

Boyd, David, A. M., Albion, Mich. (46). **I**

Boyd, Prof. James Harrington, University of Chicago, Chicago, Ill. (47). **A**

Brace, Prof. D. B., Univ. of Nebraska, Lincoln, Neb. (48).

Brace, Miss E. M., 40 Cortlandt St., Rochester, N. Y. (47).

BRACKENRIDGE, GEO. W., San Antonio, Texas. (41). **I**

Brackett, Byron B., Ph.D., 722 Union St., Schenectady, N. Y. (46). **B**

Brackett, Solomon H., St. Johnsbury, Vt. (47). **B**

Bradford, Prof. Joseph N., Ohio State Univ., Columbus, Ohio (48).

BRADLEY, ARTHUR C., Newport, N. H. (43).

Bradley, Charles S., 44 Broad St., New York, N. Y. (40).

BRADLEY, M. J., 36 Hart St., Brooklyn, N. Y. (43).

BRADLEY, MILTON, Springfield, Mass. (44). **B**

Bramwell, Geo. W., 335 Broadway, New York, N. Y. (43). **D**

Brassill, Miss Sarah Ellen, So. Weymouth, Mass. (47). **G F**

Brayton, Miss Sarah H., M.D., Evanston, Ill. (33).

Brice, Judge Albert G., 19 Camp St., New Orleans, La. (32). **H**

Briggs, Lyman J., Dep't of Agriculture, Washington, D. C. (48). **B**

Brigham, Prof. Albert P., Hamilton, Madison Co., N. Y. (41).

Britton, Wiley, Bureau of Pensions, Springfield, Mo. (40). **F**

Brown, Harold W., Ohio State Univ., Columbus, Ohio (48). **B C**

Brown, Newton H., Newark, Delaware (48) **B D**

Brown, Robert Marshall, Box 75, Portsmouth, N. H. (48).

Brown, Samuel B., Morgantown, W. Va. (40). **E**

Brownell, Silas B., 71 Wall St., New York, N. Y. (36).

Browning, Philip Embury, Kent Chemical Laboratory, Yale University, New Haven, Conn. (46). **C**

Brundage, Albert H., Ph.G., M.D., 1153 Gates Ave., Brooklyn, N. Y. (43). **F G H**

Bryant, Miss D. L., 998 Spring Garden St., Greensboro, N. C. (42). **E**

Buckingham, Chas. L., 195 Broadway, New York, N. Y. (28).

Buffum, Prof. Burt C., State University, Laramie, Wyo. (42). **G**

Burgess, Edward S., 11 W. 88th St., New York, N. Y. (47).

Burnham, Edward J., Manchester, N. H. (47). **F**

Burr, Mrs. Laura E., Commercial Hotel, Lansing, Mich. (34). **B**

Burton, Prof. Alfred E., Massachusetts Institute of Technology, Boston, Mass. (40). **E**

Burwell, Arthur W., Ph.D., 208 Superior St., Cleveland, Ohio. (37).

Calkins, Dr. Marshall, Springfield, Mass. (29).

Calvert, Prof. Sidney, M.A., University of Missouri, Columbia, Mo. (47). **C**

Canfield, James Hulme, Pres. Ohio State Univ., Columbus, Ohio. (48). **I**

Card, Fred W., Kingston, R. I. (45). **G**

Carpenter, Ford A., U. S. Weather Bureau, San Diego, Cal. (44). **B**

Carpenter, Geo. O., Jr., care St. Louis Lead and Oil Co., St. Louis, Mo. (29).

CARTER, JAMES C., 277 Lexington Ave., New York, N. Y. (36).

Carter, John E., Knox and Coulter Sts., Germantown, Pa. (33). **B H**

Carter, Prof. Truman Post, 872 Grove St., Jacksonville, Ill. (46). **F G**

Cary, Albert A., 95 Liberty St., New York, N. Y. (36). **D**

Cary, Mrs. Elizabeth M. L., 184 Delaware Ave., Buffalo, N. Y. (45). **E**

Case, Eckstein, 9 Rockwell St., Cleveland, Ohio. (47).

Chandler, Richard E., Stillwater, Oklahoma (46). **B D**

Chaney, Prof. Lucian W., Jr., Carleton College, Northfield, Minn. (45).

Chase, R. Stuart, 53 Summer St., Haverhill, Mass. (18). **F**

Cheney, Mrs. Ednah Dow, Jamaica Plain, Mass. (47).

Child, Clement D., Colgate Univ., Hamilton, N. Y. (44). **B**

Chittenden, Frank Hurlbut, U. S. Dep't of Agric., Washington, D. C. (48). **F**

Chrystie, Wm. F., Hastings-on-Hudson, New York, N. Y. (36).

Clancy, Michael Albert, 1426 Corcoran St., Washington, D. C. (40). **H**

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Taft, Elihu B., Burlington, Vt. (36). **H**

Talbott, Mrs. Laura Osborne, 1445 Huntington Place, Washington, D. C.
    (36). **F G I**

Taylor, Edward Randolph, Cleveland, Ohio. (39). **C**

Taylor, Prof. Jas. M., Hamilton, Madison Co., N. Y. (33). **A D**

Taylor, Robert S., Box 2019, Fort Wayne, Ind. (39).

Thaw, Mrs. Mary Copley, Pittsburg, Pa. (41). **H**

Thompson, Alton H., 721 Kansas Ave., Topeka, Kas. (33). **H**

Thompson, Fred'k F., 283 Madison Ave., New York, N. Y. (36).

Thompson, James E., New Carlisle. Ohio (48).

Thompson, J. L., M.D., Indianapolis, Ind. (39). **F**

Tight, Prof. William George, Granville, Ohio. (39). **E**

Todd, Albert M., Kalamazoo, Mich. (37). **C**

Todd, Dr. J. H., Wooster, Ohio (48).

Towle, Wm. Mason, Penna. State College, Center Co., Pa. (44). **D**

Townsend, Charles O., Maryland Agricultural College, College Park, Md.
    (46).

Treat, Erastus B., 5 Cooper Union, cor. 4th Ave. and 8th St., New York,
    N. Y. (29). **F I**

Trowbridge, Augustus, Ph.D., 812 E. Catherine St., Ann Arbor, Mich.
    (47).

Trowbridge, Rev. Chas. R., 223 Porter St., Easton, Pa. (46). **H I**

Trowbridge, Luther H., East Grand Circus Park, Detroit, Mich. (29).

Trowbridge, Mrs. M. E. D., East Grand Circus Park, Detroit, Mich. (21).
    I G

True, Rodney Howard, Wingra Park, Madison, Wis. (46). **G**

Trueblood, Mary Esther, Ph.M., Earlham College, Richmond, Ind. (47).
    A

Turner, J. Spencer, 109 Duane St., New York, N. Y. (43). **B**

Tyler, Dr. Harry Walter, 491 Boylston St., Boston, Mass. (47). **A**

Vail, Prof. Hugh D., Santa Barbara, Cal. (18).

Valentine, Edw. P., Richmond, Va. (33). **H**

Van Antwerp, Rev. Francis J., 26 Harper Ave., Detroit, Mich. (46).

Van Beuren, Frederick T., 21 W. 14th St., New York, N. Y. (36).

Van Brunt, Cornelius, Room No. 410, 310 E. 57th St., New York, N. Y.
    (28).

Van Denberg, Joseph King, Fort Edward, N. Y. (47).

Van Slyke, Lucius L., Agr. Expt. Station, Geneva, N. Y. (41).

Varney, A. L., Major of Ordnance Dept., U. S. A., San Antonio Arsenal, San Antonio, Texas (44). **H**

VAUX, GEO., Jr., 404 Girard Building, Philadelphia, Pa. (33). **E A**

Villard, Fanny G., Dobbs Ferry, N. Y. (36).

Vinal, W. Irving, 1106 East Capitol St., Washington, D. C. (40). **E**

Voorhees, Chas. H., M.D., 111 Carroll Place, New Brunswick, N. J. (29). **F H**

Wagner, George, 1120 E. University Ave., Ann Arbor, Mich. (46). **C F G**

Waldron, Clare B., Agricultural College, Fargo, N. D. (45).

Wales, Salem H., 25 E. 55th St., New York, N. Y. (36).

Walker, Byron Edmund, Toronto, Can. (38). **E**

Walker, George C., Room 519, Rookery Building, Chicago, Ill. (17).

Walker, James, Seth Thomas Clock Co., 49 Maiden Lane, New York, N. Y. (43).

Wallace, Edwin Corby, Ph.G., Foot of Sixth St., Long Island City, N. Y. (47).

Ward, Frank A., 16–26 College Ave., Rochester, N. Y. (40).

Ward, J. Langdon, 120 Broadway, New York, N. Y. (29). **I**

Ward, Robert De C., Harvard Univ., Cambridge, Mass. (47).

Wardle, Harriet N., 125 N. 10th St., Philadelphia, Pa. (47). **E**

Ware, Miss Mary L., 41 Brimmer St., Boston, Mass. (47).

Ware, Wm. R., Columbia University, New York, N. Y. (36).

Warner, Charles F., 837 State St., Springfield, Mass. (45) **B**

Warren, Howard C., Ph.D., Princeton, N. J. (46).

Warren, Mrs. Susan C., 67 Mt. Vernon St., Boston, Mass. (29).

Warrington, James N., 1711 South Hope St., Los Angeles, Cal. (34). **D A B**

WATERS, GEO. F., 6 Somerset St., Boston, Mass. (29). **B F H E D**

Watson, Miss C. A., North Andover, Mass. (31). **D**

Watson, Thomas A., Weymouth, Mass. (42). **E**

Watters, William, M.D., 26 S. Common St., Lynn, Mass. (40). **E G**

Watts, A. J., M.D., 1123 Bedford Ave., Brooklyn, N. Y. (43).

Weaver, Gerrit E. Hambleton, A.M., 203 De Kalb Square, West Philadelphia, Pa. (38). **G I**

Webber, Herbert J., U. S. Dept. of Agric., Washington, D. C. (47).

Webster, Prof. Arthur Gordon, Clark University, Worcester, Mass. (47). **B A**

Weed, J. N., 71 Water St., Newburgh, N. Y. (37). **E I**

Weeks, Fred Boughton, U. S. Geological Survey, Washington, D. C. (44). **E**

Weems, J. B., Ph.D., Iowa Agricultural College, Ames, Iowa (44). **C**

Weick, Chas. W., Columbia Univ., N. Y. (48). **D**

Weinzirl, John, 416 S. Arno St., Albuquerque, New Mex. (45). **G**

Welch, William Henry, M.D., 935 St. Paul St., Baltimore, Md. (47). **F H**

Wells, Frank, M.D., 178 Devonshire St., Boston, Mass. (47). C

Wells, Samuel, 31 Pemberton Square, Boston, Mass. (24). H

Wells, William H., Jr., Lockport, N. Y. (39). E

Wernicke, Prof. Paul, 107 E. Maxwell St., Lexington, Ky. (44). A B

Werum, Jno. H., Toledo, Ohio (40). E F

Wetzler, Jos., 203 Broadway, New York, N. Y. (36).

Wheeler, T. B., M.D., 123 Metcalfe St., Montreal, Can. (11).

Wheeler, William, C.E., Concord, Mass. (41).

White, Charles G., Lake Linden, Mich. (46). B C

White, Charles H., Medical Director U. S. N., U. S. Naval Museum of Hygiene, Washington, D. C. (34). C

White, John Williams, 18 Concord Ave., Cambridge, Mass. (47).

White, LeRoy S., Box 924, Waterbury, Conn. (23).

White, Thaddeus R., 257 W. 45th St., New York, N. Y. (42). A

White, Theodore Greely, Ph.B., A.M., Columbia University, New York, N. Y. (47). B E

Whitfield, Thomas, Ph.D., 240 Wabash Ave., Chicago, Ill. (41). C

Whiting, Mrs. Francis, 914 W. Lafayette St., Norristown, Pa. (40).

Whiting, S. B., 11 Ware St., Cambridge, Mass. (33). D

Whitney, Miss Mary W., Vassar College Observatory, Poughkeepsie, N. Y. (47). A

Whitney, Willis Rodney, 29 Fairmount, Jamestown, N. Y. (46). C

Wiegand, Karl McKay, Ithaca, N. Y. (45). G

Wilbour, Mrs. Charlotte B., Little Compton, R. I. (28).

Wilcox, Miss Emily T., 299 College St., Middletown, Conn. (33). B A

Williams, Charles B., Raleigh, N. C. (47).

Williams, Henry Smith, M.D., 165 W. 82nd St., New York, N. Y. (34). F

WILMARTH, MRS. HENRY D., 51 Eliot St., Jamaica Plain, Mass. (40).

Wilmot, Thos. J., Commercial Cable Co., Waterville, County Kerry, Ireland (27). B

Wilson, Prof. Andrew G., Lenox College, Hopkinton, Iowa (43). E

Windsor, Sarah Sweet, A.B., M.D., 138 Marlboro St., Boston, Mass. (47). F H

Wingate, Miss Hannah S., 15 W. 129th St., New York, N. Y. (31). E I

Witmer, Dr. Lightner, University of Pennsylvania, Philadelphia, Pa. (46). H

Wolcott, Mrs. Henrietta L. T., Dedham, Mass. (29).

Wolff, Frank A., Jr., Ph.D., 1028 Vermont Ave., N. W., Washington, D. C. (47). B

Wood, Mrs. Cynthia A., 171 W. 47th St., New York, N. Y. (43).

Wood, Miss Elvira, 198 Adams St., Waltham, Mass. (47).

Wood, Rob't Williams, 515 State St., Madison, Wis. (46).

WOOD, WALTER, 400 Chestnut St., Philadelphia, Pa. (33). F I

Woodbury, Charles T., North Andover, Mass. (47). C

Worcester, Dean C., Ann Arbor, Mich. (46). F H

Wright, John S., care Eli Lilly & Co., Indianapolis, Ind. (42). G

Wright, Jonathan, M.D., 73 Remsen St., Brooklyn, N. Y. (43).

HONORARY FELLOWS.[1]

ROGERS, PROF. WILLIAM B., Boston, Mass. (1). 1881. (Born Dec. 7, 1804. Died May 30, 1882.) B E

CHEVREUL, MICHEL EUGÈNE, Paris, France. (35). 1886. (Born Aug. 31, 1786. Died April 9, 1889.) C

GENTH, DR. F. A., 3937 Locust St., Philadelphia, Pa. (24). 1888. (Born May 17, 1820. Died Feb. 2, 1892.) C E

HALL, PROF. JAMES, Albany, N. Y. (1). 1890. (Born in 1811. Died Aug. 7, 1898.)

GOULD, DR. BENJAMIN APTHORP, Cambridge, Mass. (2). 1895. (Born Sept. 27, 1824. Died Nov. 26, 1896.) A B

LEUCKART, PROF. RUDOLF. (44). 1895. (Born in Helmstedt, Braunschweig, Germany, Oct. 7, 1823. Died in Leipzig, Feb. 7, 1898.) F

GIBBS, PROF. WOLCOTT, Newport, R. I. (1). 1896. C B

WARINGTON, ROBERT, F.R.S., Rothamsted, Harpenden, England (40). 1899. C

FELLOWS.[2]

Abbe, Professor Cleveland, Meteorologist, Weather Bureau, U. S. Depart. Agric., Washington, D. C. (16). 1874. B A

Abbe, Cleveland, Jr., Winthrop College, Rock Hill, S. C. (44). 1899. E

Abbe, Dr. Robert, 11 W. 50th St., New York, N. Y. (36). 1892.

ABBOT, DR. SAMUEL L., 90 Mt. Vernon St., Boston, Mass. (1).

Abert, S. Thayer, Metropolitan Club, Washington, D. C. (30). 1891. A B D E I

Adriance, John S., 105 E. 39th St., New York, N. Y. (39). 1895. C

Alden, John, Pacific Mills, Lawrence, Mass. (36). 1898.

Aldrich, Prof. William Sleeper, Univ. of Illinois, Champaign, Ill. (43). 1897. D

Alvord, Major Henry E., U. S. Depart. Agric., Washington, D. C. (29). 1882. I

Alwood, Prof. Wm. B., Agricultural and Mechanical College and Experiment Station, Blacksburg, Va. (39). 1891. F

Anderson, Alexander P., The University of Minnesota, Minneapolis, Minn. (45). 1899. G

Andrews, Prof. Launcelot W., Iowa City, Iowa. (39). 1891. C

Anthony, Prof. Wm. A., Cooper Union, New York, N. Y. (28). 1880. B

Arthur, J. C., Lafayette, Ind. (21). 1883. G

Ashmead, Wm. H., U. S. National Museum, Washington, D. C. (40). 1892. F

Atkinson, Edward, 31 Milk St., Boston, Mass. (29). 1881. I D

Atkinson, George F., Cornell University, Ithaca, N. Y. (39). 1892. G

[1] See ARTICLE VI of the Constitution.   [2] See ARTICLE IV of the Constitution.

₊ The number in parenthesis indicates the meeting at which the member joined the Association; the date following is the year when made a Fellow; the black letters at end of line are those of the sections to which the Fellow belongs.

When the name is given in small capitals, it designates that the Fellow is also a Life Member.

Atwater, Prof. W. O., Wesleyan University, Middletown, Conn.(29). 1882. **C**

Atwell, Charles B., 1938 Sherman Ave., Evanston, Ill. (36). 1890. **G**

Auchincloss, Wm. S., Hotel Beechwood, Summit, N. J. (29). 1886. **D A**

Austen, Prof. Peter T., 99 Livingston St., Brooklyn, N. Y. (44). 1896. **C**

Avery, Elroy M., Ph.D., LL.D., 657 Woodland Hills Ave., Cleveland, Ohio. (37). 1889. **B**

Ayres, Prof. Brown, Tulane University, New Orleans, La. (31). 1885. **B**

Babcock, Prof. S. Moulton, Madison, Wis. (33). 1885. **C**

Bailey, E. H. S., Lawrence, Douglas Co., Kan. (25). 1889. **C E**

Baker, Frank, M.D., 1804 Columbia Road, Washington, D. C. (31). 1886. **F H**

Baker, Marcus, U. S. Geological Survey, Washington, D. C. (30). 1882. **A**

Baldwin, Prof. J. Mark, Princeton University, Princeton, N. J. (46). 1898. **H**

Ballard, Harlan H., 50 South St., Pittsfield, Mass. (31). 1891. **E F**

Barbour, Prof. Erwin H., Univ. of Nebraska, Lincoln, Nebr. (45). 1898. **E**

BARKER, PROF. G. F., 3909 Locust St., Philadelphia, Pa. (13). 1875. **B C**

Barnard, Edward E., care Yerkes Observatory, Lake Geneva, Williams Bay P. O., Wis. (26). 1883. **A**

Barnes, Chas. Reid, Ph.D., University of Chicago, Chicago, Ill. (33). 1885. **G**

Barnum, Miss Charlotte C., Ph.D., 344 Humphrey St., New Haven, Conn. (36). 1896. **A**

Barrows, Walter B., Agricultural College, Ingham Co., Mich. (40). 1897. **F**

Bartlett, Prof. Edwin J., Dartmouth College, Hanover, N. H. (28). 1883. **C**

Bartlett, John R., Commander U. S. N., Lonsdale, R. I. (30). 1882. **E B**

Bartley, Elias H., M.D., 21 Lafayette Ave., Brooklyn, N. Y. (33). 1894. **C**

Barton, G. E., 27 Main St., Millville, N. J. (46). 1898. **C**

Barton, Samuel M., Ph.D., The University of the South, Sewanee, Tenn. (43). 1899. **A**

Barus, Carl, Ph.D., Wilson Hall, Brown University, Providence, R. I. (33). 1887. **B**

Bascom, Miss Florence, Bryn Mawr College, Bryn Mawr, Pa. (42). 1897. **E**

Baskerville, Charles, University of North Carolina, Chapel Hill, N. C. (41). 1894. **C E**

Bassett, Homer F., Waterbury, Conn. (23). 1874. **F**

Bauer, Louis A., Ph.D., U. S. C. & G. Survey, Washington, D. C. (40). 1892. **A**

Bausch, Edw., P. O. Drawer 1033, Rochester, N. Y. (26). 1883. **A B C F**

Beal, Prof. Wm. James, Agricultural College, Ingham Co., Mich. (17). 1880. **G**

Beardsley, Prof. Arthur, C.E., Ph.D., Swarthmore College, Swarthmore, Del. Co., Pa. (33). 1885. **D**

Bedell, Frederick, Ph.D., Cornell University, Ithaca, N. Y. (41). 1894. **B A**

Bell, Alex. Melville, 1525 35th St., Washington, D. C. (31). 1885. H

Bell, Robert, M.D., LL.D., F.R.S., Geological Survey, Ottawa, Can. (38). 1889. E F

Beman, Wooster W., 19 S. 5th St., Ann Arbor, Mich. (34). 1886. A

BENJAMIN, MARCUS, Smithsonian Institution, Washington, D. C. (27). 1887. C

Benjamin, Rev. Raphael, M.A., Hotel Premier, 72nd St., New York, N. Y. (34). 1887. E F G H

Benneson, Miss Cora Agnes, A.M., LL.B., 4 Mason St., Cambridge, Mass. (47). 1899. I H

Bessey, Prof. Charles E., University of Nebraska, Lincoln, Nebr. (21). 1880. G

Bethune, Rev. C. J. S., 500 Dufferin Ave., London, Ontario, Can. (18). 1875. F

Bickmore, Prof. Albert S., American Museum of Natural History, Central Park, New York, N. Y. (17). 1880. H

Bigelow, Prof. Frank H., U. S. Weather Bureau, Washington, D. C. (36). 1888. A

BIXBY, MAJOR W. H., U. S. A., Custom House, Cincinnati, Ohio. (34). 1892. D

Blair, Andrew A., 406 Locust St., Philadelphia, Pa. (44). 1896. C

Blake, Clarence J., M.D., 226 Marlborough St., Boston, Mass. (24). 1877. B F

Blake, Francis, Auburndale, Mass. (23). 1874. B A

Bleile, Albert M., M.D., State University, Columbus, Ohio (37). 1896. F

Blue, Archibald, Director of the Bureau of Mines, Toronto, Can. (35). 1890. I

Bodine, Prof. Donaldson, Wabash College, Crawfordsville, Ind. (45). 1899. E F

Bohannan, Prof. Rosser D., Ohio State Univ., Columbus, Ohio (46). 1898. A

Bolley, Henry L., North Dakota Experiment Station, Fargo, North Dakota (39). 1892. G

BOLTON, DR. H. CARRINGTON, Cosmos Club, Washington, D. C. (17). 1875. C

Bond, Geo. M., care of The Pratt & Whitney Co., Hartford, Conn. (33). 1885. D

Booth, Miss Mary A., 60 Dartmouth St., Springfield, Mass. 1894. (34). F I G

Bowditch, Charles P., 28 State St., Boston, Mass. (43). 1897. H

Bowditch, Prof. H. P., Jamaica Plain, Mass. (28). 1880. F B H

Bowser, Prof. E. A., Rutgers College, New Brunswick, N. J. (28). 1881.

Boyd, James E., Ohio State Univ., Columbus, Ohio (46). 1899. B

BOVÉ, MARTIN H., M.D., Coopersburg, Lehigh Co., Pa. (1). 1896. C

Brackett, Richard N., Clemson College, S. C. (37). 1891. C E

Bradford, Royal B., Commander U. S. N., care Navy Dep't, Washington, D. C. (31). 1891. B D

Branner, Prof. John C., Stanford University, Cal. (34). 1886. **E F**

Brashear, Jno. A., Allegheny, Pa. (33). 1885. **A B D**

Bristol, Wm. H., Stevens Institute, Hoboken, N. J. (36). 1894. **A B D**

Britton, N. L., Ph.D., 41 E. 49th St., New York, N. Y. (29). 1882. **G E**

Brooks, Wm. R., D.Sc., Director Smith Observatory, Geneva, N. Y. (35). 1886. **A B D G**

Brown, Robert, care of Yale University Observatory, New Haven, Conn. (11). 1874. **A**

Brown, Mrs. Robert, New Haven, Conn. (17). 1874.

Brühl, Gustav, cor. John and Hopkins Sts., Cincinnati, Ohio. (28). 1886. **H**

Brush, Charles F., Brush Electric Light Co., Cleveland, Ohio. (35). 1886. **B**

BRUSH, PROF. GEORGE J., Yale University, New Haven, Conn. (4). 1874. **C E**

Buckhout, W. A., State College, Centre Co., Pa. (20). 1881. **F**

Bull, Prof. Storm, Madison, Wis. (44). 1897. **D**

Burgess, Dr. Thomas J. W., Med. Sup't Protestant Hospital for the Insane, Montreal, Can. (38). 1889. **G**

Burr, Prof. William H., School of Mines, 41 E. 49th St., New York, N. Y. (31). 1883.

Butler, A. W., Secretary Board of State Charities, Indianapolis, Ind. (30). 1885. **F H**

Caldwell, Prof. Frank C., Ohio State University, Columbus, Ohio. (46). 1898. **B D**

Caldwell, Prof. George C., Cornell University, Ithaca, N. Y. (23). 1875. **C**

Calvin, Prof. Samuel, State University of Iowa, Iowa City, Iowa. (37). 1889. **E F**

Campbell, Prof. Douglas H., Stanford University, Cal. (34). 1888. **G**

Canby, William M., 1101 Delaware Avenue, Wilmington, Del. (17). 1878. **G**

Carhart, Prof. Henry S., University of Michigan, Ann Arbor, Mich. (29). 1881. **B**

Carpenter, Louis G., Agricultural College, Fort Collins, Col. (32). 1889. **A B**

Carter, James Madison G., M.D., Waukegan, Ill. (39). 1895. **F**

Casey, Thomas L., U. S. Engineer Office, Norfolk, Va. (38). 1892. **F**

Catlin, Charles A., 133 Hope St., Providence, R. I. (33). 1895. **C**

CATTELL, PROF. JAMES McKEEN, Columbia University, New York, N. Y. (44). 1896. **B F H I**

Chalmot, G. de, Holcomb Rock, Va. (44). 1896. **C**

Chamberlin, T. C., 5041 Madison Ave., Chicago, Ill. (21). 1877. **E B F H**

Chandler, Prof. C. F., School of Mines, Columbia University, New York, N. Y. (19). 1875. **C**

Chandler, Prof. Charles Henry, Ripon, Wis. (28). 1883. **A**

Chandler, Seth C., 16 Craigie St., Cambridge, Mass. (29). 1882. **A**

Chanute, O., 413 E. Huron St., Chicago, Ill. (17). 1877. D I

Chase, Frederick L., Yale University Observatory, New Haven, Conn. (43). 1896. A

Cheney, Lellen Sterling, 318 Bruen St., Madison, Wis. (42). 1894. G

Chester, Prof. Albert H., Rutgers College, New Brunswick, N. J. (29). 1882. C F

Chester, Captain Colby M., U. S. N., care Navy Department, Washington, D. C. (28). 1897. E

Chickering, Prof. J. W., Deaf Mute College, Washington, D. C. (22). 1877. G I .

Christie, James, Pencoyd, Pa. (33). 1894. D

Chute, Horatio N., Ann Arbor, Mich. (34). 1889. B C A

Clapp, Miss Cornelia M., Mt. Holyoke College, South Hadley, Mass. (31). 1883. F

Clark, Prof. John E., 445 Orange St., New Haven, Conn. (17). 1875. A

Clark, Wm. Bullock, Ph.D., Johns Hopkins University, Baltimore, Md. (37). 1891. E

Clarke, Prof. F. W., U. S. Geological Survey, Washington, D. C. (18)· 1874. C

Clarke, John Mason, Ass't State Geologist and Palæontologist of N. Y., State Hall, Albany, N. Y. (45). 1897. E

Claypole, Dr. Agnes M., Cornell Univ., Ithaca, N. Y. (46). 1899. F

Claypole, Miss Edith J., Cornell Univ., Ithaca, N. Y. (46). 1899. F

Claypole, Prof. Edward W., Polytechnic Institute, Pasadena, Cal. (30). 1882. E F

Cloud, John W., 974 Rookery, Chicago, Ill. (28). 1886. A B D

Cochran, C. B., 514 South High St., West Chester, Chester Co., Pa. (43). 1896. C

Coffin, Selden J., Lafayette College, Easton, Pa. (22). 1874. A I

Cogswell, W. B., Syracuse, N. Y. (33). 1891. D

COLBURN, RICHARD T., Elizabeth, N. J. (31). 1894. I F H

Cole, Prof. Alfred D., Denison University, Granville, Ohio (39). 1891. B C

Collin, Prof. Alonzo, Cornell College, Mount Vernon, Iowa (21). 1891. B C

Collingwood, Francis, Elizabeth, N. J. (36). 1888. D

Comstock, Prof. Geo. C., Washburn Observ., Univ. of Wisconsin, Madison, Wis. (34). 1887. A

Conant, Prof. L. L., Polytechnic Institute, Worcester, Mass. (39). 1892. A

Cook, Prof. Orator F., U. S. National Museum, Washington, D. C. (40). 1892. G

Cooley, Prof. Le Roy C., Vassar College, Poughkeepsie, N. Y. (19). 1880. B C

Cooley, Prof. Mortimer E., Univ. of Mich., Ann Arbor, Mich. (33). 1885. D

Corthell, Elmer L., 27 Pine St., New York, N. Y. (34). 1886. D I E

Cowles, Alfred H., 656 Prospect St., Cleveland, Ohio. (37). 1897. **B C**

Crafts, James Mason, Mass. Institute Technology, Boston, Mass. (47). 1898. **C**

Crampton, Chas. A., M.D., Office of Internal Revenue, U. S. Treasury Depart., Washington, D. C. (36). 1887. **C**

Crandall, Prof. Charles S., Fort Collins, Col. (40). 1894.

Crawford, Prof. Morris B., Middletown, Conn. (30). 1889. **B**

Crockett, Charles W., Rensselaer Polytechnic Institute, Troy, N. Y. (39). 1894. **A D**

Cross, Prof. Charles R., Massachusetts Institute Technology, Boston, Mass. (29). 1880. **B**

Culin, Stewart, Univ. of Pennsylvania, Philadelphia, Pa. (33). 1890. **H**

Cummings, Miss Clara E., Wellesley College, Wellesley, Mass. (47). 1899. **G**

Cushing, Frank H., Bureau of Ethnology, Washington, D. C. (40). 1893. **H**

Cushing, Henry Platt, Adelbert College, Cleveland, Ohio (33). 1888. **E**

Dall, William H., Smithsonian Institution, Washington, D. C. (18). 1874. **H F**

Dana, Edward Salisbury, New Haven, Conn. (23). 1875. **B E**

Darton, Nelson H., U. S. Geological Survey, Washington, D. C. (37). 1893.

Davenport, Charles Benedict, Ph.D., 11 Francis Ave., Cambridge Mass. (46). 1898. **F**

Davidson, Prof. George, 530 California St., San Francisco, Cal. (29). 1881. **A B D**

Davis, Bradley Moore, Dep't of Botany, University of Chicago, Chicago, Ill. (45). 1897. **G**

Davis, C. H., Commander U. S. N., Navy Department, Washington, D. C. (40). 1896.

Davis, J. J., M.D., 1119 College Ave., Racine, Wis. (31). 1899. **F G**

Davis, Prof. Wm. Morris, Cambridge, Mass. (33). 1885. **E B**

Dawson, Geo. M., S.S.C., F.G.S., Geol. Survey, Ottawa, Can. (38). 1895. **E**

Dawson, Sir William, Principal McGill College, Montreal, Can. (10). 1875. **E**

Day, David F., Buffalo, N. Y. (35). 1887. **G**

Day, Fisk, H., M.D., 309 Sycamore St., Lansing, Mich. (20). 1874. **E H F**

Dennis, Louis Monroe, Cornell University, Ithaca, N. Y. (43). 1895. **C**

Denton, Prof. James E., Stevens Institute, Hoboken, N. J. (36). 1888. **D B A**

Derby, Orville A., Sao Paulo, Brazil, S. A. (39). 1890.

Dewey, Frederic P., 702 9th St., N. W., Washington, D. C. (30). 1886. **C E**

Dewey, L. H., U. S. Dep't of Agriculture, Washington, D. C. (40). 1899. **F G**

Dimmock, George, 679 State St., Springfield, Mass. (22). 1874. F

DIXWELL, EPES S., Cambridge, Mass. (1). 1896. H F

Dodge, Charles R., U. S. Dep't Agric., Washington, D. C. (22). (1874).

Dodge, Chas. W., M.S., Univ. of Rochester, Rochester, N. Y. (39). 1898. F

Dolbear, Prof. A. Emerson, Tufts College, Mass. (20). 1880. B

Doolittle, Prof. C. L., University of Pennsylvania, Philadelphia, Pa. (25). 1885. A

Dorsey, George A., Ph.D., Field Columbian Museum, Chicago, Ill. (39). 1892. H

Dorsey, N. Ernest, Ph.D., Annapolis Junction, Md. (46). 1898. B

Douglass, Andrew E., Amer. Mus. of Nat. History, Central Park, New York, N. Y. (31). 1885. H

DRAPER, DAN'L, Ph.D., N. Y. Meteorological Observatory, Central Park, 64th St., Fifth Ave., New York, N. Y. (29). 1881. B D F A

Drown, Prof. Thos. M., Lehigh University, South Bethlehem, Pa. (29). 1881. C

DU BOIS, PROF. AUG. J., New Haven, Conn. (30). 1882. A B D

Du Bois, Patterson, 1031 Walnut St., Philadelphia, Pa. (33). 1887. H C I

Dudley, Charles B., Drawer 334, Altoona, Pa. (23). 1882. C B D

DUDLEY, WM. L., Vanderbilt University, Nashville, Tenn. (28). 1881. C

Dudley, Prof. Wm. R., Stanford University, Cal. (29). 1883. G

Dumble, E. T., 1708 Prairie Ave., Houston, Tex. (37). 1891. E

Dunham, Edw. K., 338 E. 26th St., New York, N. Y. (30). 1890.

Dunnington, Prof. F. P., University Station, Charlottesville, Va. (26). 1880. C

Du Pont, Francis G., Wilmington, Del. (33). 1896. A B D

Durand, Elias J., D.S., 402 Eddy St., Ithaca, N. Y. (41). 1899. G

Durand, Prof. W. F., Ph.D., Cornell University, Ithaca, N. Y. (37). 1890. B

Dwight, Prof. William B., Vassar College, Poughkeepsie, N. Y. (30). 1882. E F

Dyar, Harrison G., A.M., Ph.D., U. S. National Museum, Washington, D. C. (43). 1898.

Earle, F. S., Polytechnic Inst., Auburn, Ala. (39). 1896. G

Eastman, Charles Rochester, Mus. Comp. Zoology, Cambridge, Mass. (41). 1896. E F

Eastman, Prof. J. R., Andover, N. H. (26). 1879. A

Eccles, Robert G., M.D., 191 Dean St., Brooklyn, N. Y. (31). 1894. F C

Eddy, Prof. H. T., University of Minnesota, Minneapolis, Minn. (24). 1875. A B D

Edison, Thos. A., Orange, N. J. (27). 1878. B

Egleston, Prof. Thomas, 35 W. Washington Square, New York, N. Y. (27). 1879. C D E

Eichelberger, William Snyder, Ph.D., Nautical Almanac Office, U. S. Naval Observatory, Washington, D. C. (41). 1896. A

Eigenmann, Carl H., Bloomington, Ind. (48). 1899. F

Eimbeck, William, U. S. C. and G. Survey, Washington, D. C. (17). 1874. A B D

Elkin, William L., Yale University Observatory, New Haven, Conn. (33). 1885. **A**

Ely, Theo. N., Chief of Motive Power, Pennsylvania R. R., Broad St. Station, Philadelphia, Pa. (29). 1886.

Emerson, Prof. Benjamin K., Box 203, Amherst, Mass. (19). 1877. **E F**

EMERSON, PROF. C. F., Box 499, Hanover, N. H. (22). 1874. **B A**

Emery, Albert H., Stamford, Conn. (29). 1884. **D B**

EMMONS, S. F., U. S. Geological Survey, Washington, D. C. (26). 1879. **E**

Engelman, George J., M.D., 336 Beacon St., Boston, Mass. (25). 1875. **F H**

Ewell, Ervin E., 3644 13th St., N. W., Washington, D. C. (40). 1896. **C**

Eyerman, John, "Oakhurst," Easton, Pa. (33). 1889. **E C**

Fairbanks, Henry, Ph.D., St. Johnsbury, Vt. (14). 1874. **B D A**

Fairchild, David Grandison, Dept. of Agric., Washington, D. C. (47). 1898. **G**

Fairchild, Prof. H. L., University of Rochester, Rochester, N. Y. (28). 1883. **E F**

Fanning, John T., Consulting Engineer, Kasota Block, Minneapolis, Minn. (29). 1885. **D**

Fargis, Rev. Geo. A., Boston College, 761 Harrison Ave., Boston, Mass. (40). 1892.

Farlow, Dr. W. G., 24 Quincy St., Cambridge, Mass. (20). 1875. **G**

Farquhar, Henry, Dep't Agric., Washington, D. C. (33.) 1886. **A I G B**

Felt, Dr. Ephraim Porter, State Entomologist, Albany, N. Y. (44). 1899. **F**

Fernow, Bernhard E., Director N. Y. S. College of Forestry, Cornell University, Ithaca, N. Y. (31). 1887. **G I**

Ferry, Ervin S., University of Wisconsin, Madison, Wis. (41). 1896.

Firmstone, F., Easton, Pa. (33). 1887. **D**

Fitz, George W., M.D., 7 Scott St., Cambridge, Mass. (47). 1898. **H**

Flather, Prof. John J., 316 Tenth Ave., S. E., Minneapolis, Minn. (44). 1896. **D**

Fletcher, Miss Alice C., care Peabody Museum, Cambridge, Mass. (29). 1883. **H**

Fletcher, Dr. Robert, Army Medical Museum, Washington, D. C. (29). 1881. **F H**

Flint, Albert S., Washburn Observatory, Madison, Wis. (30). 1887. **A**

Flint, James, M., Surgeon U. S. N., Smithsonian Institution, Washington, D. C. (28). 1882. **F**

Ford, Prof. D. R., Elmira, N. Y. (41). 1894. **A B**

Fox, Oscar C., U. S. Patent Office, Washington, D. C. (36). 1891. **B D A**

Franklin, Mrs. C. Ladd, 1507 Park Ave., Baltimore, Md. (47). 1899. **H**

Franklin, William S., Lehigh University, So. Bethlehem, Pa. (36). 1892. **B**

FRAZER, DR. PERSIFOR, Drexel Building, Room 1042, Philadelphia, Pa. (24). 1879. **E C**

Frazier, Prof. B. W., The Lehigh University, So. Bethlehem, Pa. (24). 1882. **E C**

Frear, William, State College, Center Co., Pa. (33). 1886. C

Freer, Prof. Paul C., Ann Arbor, Mich. (39). 1891. C

French, Prof. Thomas, Jr., Ridgeway Ave., Avondale, Cincinnati, Ohio (30). 1883. B

Fries, Dr. Harold H., 92 Reade St., New York, N. Y. (40). 1898. C

Frisby, Prof. Edgar, U. S. N. Observatory, Washington, D. C. (28). 1880. A

Frost, Edwin Brant, Yerkes Observatory, Williams Bay, Wis. (38). 1890. A B

Fuller, Prof. Homer T., President Drury College, Springfield, Mo. (35). 1891. C E

Fulton, Robert B., Chancellor University of Mississippi, University, Miss. (21). 1887. B A

Furness, Miss Caroline E., Vassar College Observatory, Poughkeepsie, N. Y. (47). 1899. A

Gaffield, Thomas, 54 Allen St., Boston, Mass. (29). 1889. C B

Gage, Prof. Simon Henry, Ithaca, N. Y. (28). 1881. F

Galbraith, Prof. John, Toronto, Can. (38). 1889. D

Gibbs, Prof. J. Willard, New Haven, Conn. (33). 1885. B

Gilbert, G. K., U. S. Geological Survey, Washington, D. C. (18). 1874. E

Gill, Adam Capen, Cornell University, Ithaca, N. Y. (38). 1894. E

Gill, Augustus Herman, Massachusetts Institute Technology, Boston, Mass. (44). 1896. C

Gill, Prof. Theo., Columbian University, Washington, D. C. (17). 1874. F

Gillman, Henry, 107 Fort St. West, Detroit, Mich. (24). 1875. H F G

Gilman, Daniel C., President Johns Hopkins University, Baltimore, Md. (10). 1875. E H

Glenn, William, 1348 Block St., Baltimore, Md. (33). 1893. C

Goessman, Prof. C. A., Massachusetts Agricultural College, Amherst, Mass. (18). 1875. C

Goff, E. S., 1113 University Ave., Madison, Wis. (35). 1889.

Gold, Theodore S., West Cornwall, Conn. (4). 1887. B C

Golden, Miss Katherine E., Lafayette, Ind. (42). 1897. G

Goldschmidt, S. A., Ph.D., 43 Sedgwick St., Brooklyn, N. Y. (24). 1880. C E B

Goldsmith, Edw., 658 N. 10th St., Philadelphia, Pa. (29). 1892. C B

Gooch, Frank A., Yale University, New Haven, Conn. (25). 1880. C

Goodale, Prof. G. L., Botanic Gardens, Cambridge, Mass. (18). 1875. G

Goodspeed, Arthur Willis, A.B., Ph.D., University of Pennsylvania, Philadelphia, Pa. (47). 1898. A B

Goss, Prof. Wm. F. M., Lafayette, Ind. (39). 1896.

GRANT, MRS. MARY J., 36 Division St., Danbury, Conn. (23). 1874. A

Gratacap, L. P., Ph.B., 77th St. and 8th Ave., New York, N. Y. (27). 1884. C E F

Gray, Prof. Thomas, Terre Haute, Ind. (38). 1889. D

Green, Arthur L., Purdue University, Lafayette, Ind. (33). 1888. C

Greenman, Jesse M., 875 Doan St., Cleveland, Ohio, (47). 1899. G

Grimes, J. Stanley, 1421 Wesley Ave., Evanston, Ill. (17). 1874. **E H**

Grindley, Harry Sands, 918 W. Green St., Urbana, Ill. (46). 1898. **C**

Grinnell, George Bird, 346 Broadway, New York, N. Y. (25). 1885. **F E**

Griswold, Leon Stacy, 238 Boston St., Dorchester, Mass. (38). 1893. **E**

Gruener, Hippolyte, Adelbert College, Cleveland, Ohio (44). 1898.

Gunckel, Lewis W., 435 W. Second St., Dayton, Ohio (41). 1897. **C**

Guthe, Karl E., Ph.D., 904 S. State St., Ann Arbor, Mich. (45). 1897. **B D**

Hagar, Stansbury, 31 Nassau St., New York, N. Y. (43). 1899.

Hague, Arnold, U. S. Geological Survey, Washington, D. C. (26). 1879. **E**

Haines, Reuben, Haines and Chew Sts., Germantown, Philadelphia, Pa. (27). 1889. **C B**

Hale, Albert C., Ph.D., 551 Putnam Ave., Brooklyn, N. Y. (29). 1886. **C B**

Hale, Geo. E., Yerkes Observatory, Williams Bay, Wis. (37). 1891. **A B C**

Hale, William H., Ph.D., 40 First Place, Brooklyn, N. Y. (19). 1874. **I F H C B E A**

Haliburton, R. G., Q. C., 143 John St., Toronto, Can. (43). 1895.

Hall, Arthur G., 1036 Oakland Ave., Ann Arbor, Mich. (41). 1896. **A B**

Hall, Prof. Asaph, 12 Kirkland Place, Cambridge, Mass. (25). 1877. **A**

Hall, Asaph, Jr., University of Michigan, Ann Arbor, Mich. (38). 1890. **A**

Hall, Prof. C. W., Dean College Engineering Met. and Mechan. Arts, University of Minnesota, Minneapolis, Minn. (28). 1883. **D E**

Hall, Prof. Edwin H., 5 Avon St., Cambridge, Mass. (29). 1881. **B**

Hall, Prof. Lyman B., Haverford College, Haverford, Pa. (31). 1884. **C**

Hallock, Albert P., Ph.D., 440 First Ave., New York, N. Y. (31). 1896. **C**

Hallock, Dr. William, Columbia University, New York, N. Y. (40). 1893. **B E**

Hallowell, Prof. Susan M., Wellesley College, Wellesley, Mass. (33). 1890. **G**

Halsted, Byron D., New Jersey Agricultural Experiment Station, New Brunswick, N. J. (29). 1883. **G**

Halsted, Prof. George Bruce, Austin, Texas (43). 1896.

Hambach, Dr. G., 1319 Lami St., St. Louis, Mo. (26). 1891. **E F**

Hamlin, Dr. A. C., Bangor, Me. (10). 1874. **C E H**

HANAMAN, C. E., Troy, N. Y. (19). 1883. **F**

Hardy, Prof. A. S., Dartmouth College, Hanover, N. H. (28). 1883. **A**

Hargitt, Prof. Charles W., Syracuse University, Syracuse, N. Y. (38). 1891. **F**

HARKNESS, PROF. WILLIAM, U. S. N. Observatory, Washington, D. C. (26). 1878. **A B C D**

Harper, Charles A., Ph.D., 2139 Gilbert Ave., Cincinnati, Ohio (40). 1899. **C**

Harper, Henry Winston, M.D., The Univ. of Texas, Austin, Texas (45). 1899. **C**

Harris, Abram Winegardner, Sc.D., President Maine State College, Orono, Me. (40). 1895. **C**

Harris, Prof. E. P., Amherst College, Amherst, Mass. (44). 1896.

Harris, Rollin Arthur, U. S. C. and G. Survey, Washington, D. C. (47). 1899. **A B**

Harris, Uriah R., Lieutenant Com'dr, U. S. N., care Navy Dep't, Washington, D. C. (34). 1886. **A**

Hart, Edw., Ph.D., Lafayette College, Easton, Pa. (33). 1885. **C**

Haskell, Eugene E., U. S. Engineer Office, Telephone Building, Detroit, Mich. (39). 1896. **A B D**

HASTINGS, C. S., Sheffield Scientific School of Yale University, New Haven, Conn. (25). 1878. **B**

Hayford, John F., C.E., U. S. C. and G. Survey, Washington, D. C. (46). 1898. **A B D**

Haynes, Prof. Henry W., 239 Beacon St., Boston, Mass. (28). 1884. **H**

Hedrick, Henry B., A.B., Nautical Almanac Office, U. S. Naval Observatory, Washington, D. C. (40). 1896. **A**

Hering, Rudolph, 100 William St., New York, N. Y. (33). 1885. **D E I**

Herty, Chas. Holmes, Ph.D., University of Georgia, Athens, Ga. (42). 1895. **C**

Hervey, Rev. A. B., Bath, Me. (22). 1879. **F**

Hilgard, Prof. E. W., University of California, Berkeley, Cal. (11). 1874. **C E B**

Hill, John Edward, Brown University, Providence, R. I. (44). 1897. **D**

Hill, Robert Thomas, U. S. Geological Survey, Washington, D. C. (36). 1889. **E**

Hillyer, Homer W., Ph.D., Chemical Laboratory, Univ. of Wisconsin, Madison, Wis. (42). 1896. **C**

Himes, Prof. Charles F., Carlisle, Pa. (29). 1882. **B C**

Hinrichs, Dr. Gustavus, 3132 Lafayette Ave., St. Louis, Mo. (17). 1874. **C B**

Hitchcock, Albert Spear, Manhattan, Kan. (39). 1892. **G**

HITCHCOCK, PROF. CHARLES H., Hanover, N. H. (11). 1874. **E**

Hitchcock, Romyn, Room 1804, 20 Broad St., New York, N. Y. (47). 1898. **C B**

Hobbs, William Herbert, Ph.D., Madison, Wis. (41). 1893. **E**

Hodgkins, Prof. H. L., Columbian University, Washington, D. C. (40). 1896. **A B**

HOFFMANN, Dr. FRIEDRICH, Charlottenburg, Kant St. 125, Berlin, Germany (28). 1881. **C F**

Holland, W. J., D.D., LL.D., Chancellor Western University of Pennsylvania, Pittsburg, Pa. (37). 1896. **F**

Hollick, Arthur, Columbia University, New York, N. Y. (31). 1892. **G E**

Holmes, Prof. Jos. A., Chapel Hill, N. C. (33). 1887. **E F**

Holmes, Wm. H., National Museum, Washington, D. C. (30). 1883. **H**

Hopkins, A. D., Experiment Station, Morgantown, W. Va. (42). 1899. **F**

Hopkins, Thos. C., State College, Center Co., Pa. (38). 1898. **E**

Horsford, Miss Cornelia, 27 Craigie St., Cambridge, Mass. (46). 1897. **H**

Hough, Prof. G. W., Northwestern University, Evanston, Ill. (15). 1874. **A**

Hough, Walter, U. S. National Museum, Washington, D C. (38). 1890.

Hovey, Edmund O., Amer. Mus. Nat. History, Central Park, New York, N. Y. (36). 1895. **C E**

Hovey, Rev. Horace C., 60 High St., Newburyport, Mass. (29). 1883. **E H**

Howard, Prof. Curtis C., 97 Jefferson Ave., Columbus, Ohio. (38). 1892. **C**

Howard, Leland O., U. S. Dep't Agric., Washington, D. C. (37). 1889. **F**

Howe, Charles S., Case School of Applied Science, Cleveland, Ohio. (34). 1891. **A**

Howe, Prof. Jas. Lewis, Washington and Lee University, Lexington, Va. (36). 1888. **C**

Howell, Edwin E., 612 17th St., N. W., Washington, D. C. (25). 1891. **E**

Hrdlicka, Alës, M.D., 130 E. 93d St., New York, N. Y. (46). 1897. **H**

Hubbard, Prof. Oliver Payson, 117 W. 55th St., New York, N. Y. (1). 1896.

Hunter, Andrew Frederick, Barrie, Ontario, Can. (38). 1896. **B H I**

Hunter, Prof. Joseph Rufus, Richmond College, Richmond, Va. (45). 1899. **C**

Hyatt, Prof. Alpheus, Natural History Society, Boston, Mass. (18). 1875. **E F**

Hyde, Prof. E. W., Station D, Cincinnati, Ohio. (25). 1881. **A**

Hyde, John, Statistician U. S. Dep't Agric., Washington, D. C. (47). 1898. **I E**

Iddings, Joseph P., University of Chicago, Chicago, Ill. (31). 1884. **E**

Iles, George, 5 Brunswick St., Montreal, Can. 31. (1898). **I**

Ives, Frederick E., 2750 N. 11th St., Philadelphia, Pa. (44). 1898. **B**

Jack, John G., Jamaica Plain, Mass. (31). 1890. **G**

Jackson, Prof. Charles L., Harvard University, Cambridge, Mass. (44). 1895. **C**

Jackson, Dr. Robert T., 33 Gloucester St., Boston, Mass. (37). 1890. **F**

Jacobus, David S., Stevens Institute, Hoboken, N. J. (36). 1889. **D B A**

Jacoby, Harold, Columbia University, New York, N. Y. (38). 1891. **A**

Jacoby, Henry S., Cornell University, Ithaca, N. Y. (36). 1892. **D**

Jaques, Capt. William H., Little Boarshead, N. H. (47). 1899. **D**

Jastrow, Dr. Jos., University of Wisconsin, Madison, Wis. (35). 1887. **H F**

Jayne, Horace F., 1826 Chestnut St., Philadelphia, Pa. (29). 1884. **F H**

Jeffries, B. Joy, M.D., 15 Chestnut St., Boston, Mass. (29). 1881. **F H**

Jenkins, Edw. H., Drawer 101, New Haven, Conn. (33). 1885. **C**

Jenks, Elisha T., Middleborough, Mass. (22). 1874. **D**

Jennings, Walter L., 900 Beacon St., Boston, Mass. (45). 1898.

Jesup, Prof. Henry G., Dartmouth College, Hanover, N. H. (36). 1891. **F**

Jesup, Morris K., 44 Pine St., New York. N. Y. (29). 1891. **I**

Jewett, Prof. Frank Fanning, Oberlin College, Oberlin, Ohio (47). 1899. **C**

Johnson, John B., Washington University, St. Louis, Mo. (33). 1886. D
Johnson, Otis C., 730 Thayer St., Ann Arbor, Mich. (34). 1886. C
Jones, Lewis R., Burlington, Vt. (41). 1894. G
Jordan, Prof. David S., Stanford University, Cal. (31). 1883. F
Julien, A. A., New York Academy of Sciences, New York, N. Y. (24).
    1875. E C
Kahlenberg, Louis, Ph.D., 306 Lake St., Madison, Wis. (46). 1898. C
Kastle, Prof. J. H., State College of Kentucky, Lexington, Ky. (45).
    1898. C
Kedzie, Prof. Nellie S., Peoria, Ill. (34). 1890. I F
Kedzie, Prof. Robert C., Agricultural College, Mich. (29). 1881. C
Keep, Wm. J., Detroit, Mich. (37). 1897.
Kellerman, Prof. William A., Ohio State University, Columbus, Ohio
    (41). 1893. G
Kemp, James F., School of Mines, Columbia University, New York,
    N. Y. (36). 1888. E
Kendrick, Prof. Arthur, Rose Polytechnic Institute, Terre Haute, Ind.
    (45). 1897. B
Kent, William, Passaic, N. J. (26). 1881. D I
Kenyon, Frederick C., U. S. Dep't Agric., Washington, D. C. (46). 1897. F
Kershner, Prof. Jefferson E., Lancaster City, Pa. (29). 1883. A B
Kinealy, John H., Washington University, St. Louis, Mo. (36). 1891. D
King, F. H., Experiment Station, Madison, Wis. (32). 1892. E F
Kingsbury, Prof. Albert, 4 Marston Way, Worcester, Mass. (43). 1898. D
Kingsbury, Benj. F., Ithaca, N. Y. (45). 1899. F
Kinnicutt, Dr. Leonard P., Polytechnic Institute, Worcester, Mass. (28).
    1883. C
Klotz, Otto Julius, 437 Albert St., Ottawa, Can. (38). 1889.
Kober, Geo. Martin, M.D., 1819 Q St., N. W., Washington, D. C. (40).
    1896. H
Kunz, G. F., Care Messrs. Tiffany & Co., Union Square, New York, N. Y.
    (29). 1883. E H C
Lacoe, Ralph D., Pittston, Pa. (31). 1893. E F
Ladd, Prof. E. F., Agricultural College, Fargo, No. Dakota (36). 1889. C
Laflamme, Prof. J. C. K., Laval University, Quebec, Can. (29). 1887. E B
LaFlesche, Francis, Indian Bureau, Interior Dep't, Washington, D. C.
    (33). 1885. H
Lamb, Daniel S., M.D., 800 10th St., N. W., Washington, D. C. (40).
    1894. H
Lambert, Preston A., 215 S. Center St., Bethlehem, Pa. (41). 1896. A
Landreth, Olin H., Union College, Schenectady, N. Y. (28). 1883. D
Langley, Prof. S. P., Smithsonian Institution, Washington, D. C. (18).
    1874. A B
Lanza, Prof. Gaetano, Massachusetts Institute of Technology, Boston,
    Mass. (29). 1882. D A B
Lattimore, Prof. S. A., University of Rochester, Rochester, N. Y. (15).
    1874. C

Laudy, Louis H., Ph.D., School of Mines, Columbia University, New York, N. Y. (28). 1890. C

Lazenby, Prof. Wm. R., Columbus, Ohio. (30). 1882. B I

Leach, Miss Mary F., Mt. Holyoke College, South Hadley, Mass. (44). 1896. C

LeConte, Prof. Joseph, University of California, Berkeley, Cal. (29). 1881. E F

Ledoux, Albert R., Ph.D., 9 Cliff St., New York, N. Y. (26). 1881. C

Lefavour, Prof. Henry, Williams College, Williamstown, Mass. (42). 1894.

Lehmann, G. W., Ph.D., 412 E. Lombard St., Baltimore, Md. (30). 1885. C B

Lennon, William H., Brockport, N. Y. (31). 1894. G C

Lesley, Prof. J. Peter, P. O. Box 93, Milton, Mass. (2). 1874. E

Leverett, Frank, Denmark, Iowa (37). 1891. E

Libbey, Prof. William, Princeton, N. J. (29). 1887. E F

Lindenkohl, Adolphus, U. S. C. and G. Survey, Washington, D. C. (40). 1898. E

Lindenthal, Gustav, C.E., 45 Cedar St., New York, N. Y. (37). 1891. I

Lloyd, John Uri, Court and Plum Sts., Cincinnati, Ohio (38). 1890. C F

Loeb, Morris, Ph.D., 118 West 72d St., New York, N. Y. (36). 1889. C

Lord, Prof. Nat. W., State University, Columbus, Ohio (29). 1881. C

LOUBAT, LE DUC DE, 47 rue Dumont d'Urville, Paris, France (46). 1897. H

Loud, Prof. Frank H., 1203 N. Tejon St., Colorado Springs, Colo. (29). 1890. A B

Loughridge, Dr. R. H., University of California, Berkeley, Cal. (21). 1874. E C

Love, Edward G., 80 E. 55th St., New York, N. Y. (24). 1882. C

Low, Seth, President Columbia University, New York, N. Y. (29). 1890.

Lowell, Augustus, 53 State St., Boston, Mass. (29). 1898.

Lowell, Percival, 53 State St., Boston, Mass. (36). 1896. A

Ludlow, Lt. Col. William, Corps of Eng. U. S. A., Army Building, New York, N. Y. (33). 1897. D B

Lyford, Edwin F., Springfield, Mass. (33). 1896. B C H

Lyle, David A., Major U. S. A., Ordnance Office, Washington, D. C. (28). 1880. D

Lyon, Dr. Henry, 34 Monument Sq., Charlestown, Mass. (18). 1874.

McClintock, Emory, Morristown, N. J. (43). 1895. A

McCreath, Andrew S., 223 Market St., Harrisburg, Pa. (33). 1889. C E

McCurdy, Chas. W., Ph.D., University of Idaho, Moscow, Idaho (35). 1895. F E

McDonnell, Prof. Henry B., College Park, Md. (40). 1893. C

MacDougal, Daniel T., New York Botanic Garden, Bronx Park, New York, N. Y. (44). 1897. G

McGee, Dr. Anita Newcomb, Bureau of American Ethnology, Washington, D. C. (37). 1892. H

McGee, W J, Bureau of American Ethnology, Washington, D. C. (27). 1882. H E

McGill, John T., Ph.D., Vanderbilt Univ., Nashville, Tenn. (36). 1888. C

McGregory, Prof. J. F., Colgate Univ., Hamilton, N. Y. (35). 1892. C

McMahon, James, Ithaca, N. Y. (36). 1891. A

McMurtrie, William, 100 William St., New York, N. Y. (22). 1874. C

McNeill, Malcolm, Lake Forest, Ill. (32). 1885. A

McPherson, Prof. William, Ohio State Univ., Columbus, Ohio (45). 1898. C

Mabery, Prof. C. F., Case School of Applied Science, Cleveland, Ohio (29). 1881. C

Macbride, Prof. Thomas H., Iowa City, Iowa (38). 1890. G

Macfarlane, Prof. A., Lehigh University, S. Bethlehem, Pa. (34). 1886. B A

Macloskie, Prof. George, Princeton University, Princeton, N. J. (25). 1882. F G

Magie, Prof. Wm. F., Princeton University, Princeton, N. J. (35). 1887.

Maltby, Margaret E., Ph.D., 32 Eliot St., Jamaica Plain, Mass. (46). 1898.

MANN, B. PICKMAN, 1918 Sunderland Place, Washington, D. C. (22). 1874. I F

Marindin, Henry Louis, U. S. C. and G. Survey, Washington, D. C. (40). 1898. E

Mark, Prof. E. H., Louisville, Ky. (39). 1893. B

Marlatt, Charles L., Depart. Agric., Washington, D. C. (40). 1895. F

Marsh, Prof. C. Dwight, Ripon, Wis. (34). 1893. F E

Martin, Artemas, U. S. Coast Survey, Washington, D. C. (38). 1890. A

Martin, Prof. Daniel S., 126 Macon St., Brooklyn, N. Y. (23). 1879. E F

Marvin, C. F., U. S. Weather Bureau, Washington, D. C. (39). 1892. B

Marvin, Frank O., University of Kansas, Lawrence, Kan. (35). 1894. D

Mason, Dr. William P., Rensselaer Polytechnic Institute, Troy, N. Y. (31). 1886. C

Matthews, Dr. Washington, 21 South Ingalls St., Ann Arbor, Mich. (37). 1888. H

Meehan, Thomas, Germantown, Pa. (17). 1875. G

Mees, Prof. Carl Leo, Rose Polytechnic Institute, Terre Haute, Ind. (24). 1876. B C

Mell, Prof. P. H., Polytechnic Institute, Auburn, Ala. (39). 1895. E G

Mendenhall, Prof. T. C., President Worcester Polytechnic Institute, Worcester, Mass. (20). 1874. B

Mercer, H. C., Doylestown, Bucks Co., Pa. (41). 1893. H

MERRILL, FREDERICK, J. H., Ph.D., New York State Museum, Albany, N.Y. (35). 1887. E

Merriman, C. C., 1910 Surf St., Lake View, Chicago, Ill. (29). 1880. F

Merriman, Mansfield, Lehigh Univ., South Bethlehem, Pa. (32). 1885. A D I

Merritt, Ernest, Ithaca, N. Y. (33). 1890. B

Metcalf, William, Pittsburg, Pa. (33). 1894. D

Metzler, William H., Ph.D., Syracuse University, Syracuse, N. Y. (45). 1899. A

Michael, Mrs. Helen Abbott, 44 Mt. Vernon St., Boston, Mass. (33). 1885. **C F**

Miller, Prof. Arthur M., State College, Lexington, Ky. (45). 1898. **E**

Miller, Prof. Dayton C., Case School of Applied Science, Cleveland, Ohio (44). 1898. **B**

Miller, George A., 11 Cook St., Ithaca, N. Y. (46). 1898. **A**

Miller, Prof. William S., Univ. of Wisconsin, Madison, Wis. (42). 1894. **F**

Mills, Prof. Wesley, McGill College, Montreal, Can. (31). 1886. **F H**

Minot, Dr. Charles Sedgwick, Harvard Medical School, Boston, Mass. (28). 1880. **F**

Minot, Francis, M.D., Readville, Mass. (29). 1884.

Mohr, Dr. Charles, Mobile, Ala. (40). 1895. **G**

Moler, Geo. S., 106 University Ave., Ithaca, N. Y. (38). 1892.

Moody, Robert O., M.D., Fair Haven Heights, New Haven, Conn. (35). 1892. **F**

Moore, Burton E., Lincoln, Nebr. (41). 1899. **B**

Moore, Clarence B., 1321 Locust St., Philadelphia, Pa. (44). 1897. **H**

Moore, Prof. J. W., M.D., Lafayette College, Easton, Pa. (22). 1874. **B D A**

Moore, Prof. Willis L., Weather Bureau, Dep't Agric., Washington, D. C. (44). 1897. **B**

Moorehead, Warren K., Xenia, Ohio (38). 1890. **H**

Moreland, Prof. S. T., Lexington, Va. (33). 1894. **B D**

Morley, Prof. Edward W., 23 Cutler St., Cleveland, Ohio (18). 1876. **C B E**

Morse, Prof. E. S., Salem, Mass. (18). 1874. **F H**

Morton, H., Stevens Institute Technology, Hoboken, N. J. (18). 1875. **B C**

Moser, Lieut. Comd'r Jeff. F., U. S. N., Com'dg U. S. F. S. Str. Albatross, Navy Pay Office, San Francisco, Cal. (28). 1889. **E**

Moses, Dr. Thomas F., Worcester Lane, Waltham, Mass. (25). 1883. **H F**

Munroe, Prof. C. E., Columbian Univ., Washington, D. C. (22). 1874. **C**

Munson, Prof. Welton M., Maine State College, Orono, Me. (41). 1899. **G F**

Munsterberg, Hugo, Harvard University, Cambridge, Mass. (47). 1898.

Murdoch, John, Public Library, Boston, Mass. (29). 1886. **F H**

Murray, Dan'l A., Ph.D., Cornell University, Ithaca, N. Y. (47). 1899. **A**

Myers, John A., Rooms 124 and 125, Anderson Building, 12-16 John St., New York, N. Y. (30). 1889. **C**

Myers, William S., M.Sc., F.C.S., Rutgers College, New Brunswick, N. J. (43). 1898. **C**

Nagle, Prof. James C., A. and M. College, College Station, Texas (40). 1893. **D B**

Nardroff, Ernest R. von, 360½ Tompkins Ave., Brooklyn, N. Y. (44). 1896. **B**

Nef, J. U., University of Chicago, Chicago, Ill. (39). 1891. **C**

Nelson, Prof. A. B., Centre College, Danville, Ky. (30). 1882. **A B D**

Newcomb, Harry T., Census Bureau, Washington, D. C. (47). 1898.

Newcomb, Prof. S., Navy Dep't, Washington, D. C. (13). 1874. **A B**

Newcombe, Frederick Chas., 51 E. Liberty St., Ann Arbor, Mich. (43). 1896. **G**

Newell, William Wells, Editor Journal American Folk Lore, Cambridge, Mass. (41). 1893. **H**

Nichols, Ernest Fox, Hamilton, N. Y. (41). 1893. **B**

Nichols, E. L., Ph.D., Cornell University, Ithaca, N. Y. (28). 1881. **B C**

Niles, Prof. W. H., Cambridge, Mass. (16). 1874. **E**

Nipher, Prof. F. E., Washington University, St. Louis, Mo. (24). 1876. **B**

Nolan, Edw. J., M.D., Academy of Natural Sciences, Philadelphia, Pa. (29). 1890. **F**

NORTON, PROF. THOMAS H., University of Cincinnati, Cincinnati, Ohio (35). 1887. **C**

Novy, Dr. Frederick G., University of Michigan, Ann Arbor, Mich. (36). 1889. **C**

Noyes, Prof. Arthur A., Massachusetts Institute Technology, Boston, Mass. (45). 1897. **C**

Noyes, Miss Mary C., Ph.D., Lake Erie College, Painesville, Ohio (43). 1896. **B**

Noyes, Prof. Wm. A., Rose Polytechnic Institute, Terre Haute, Ind. (32). 1885. **C**

Nuttall, Mrs. Zelia, care Messrs. Bassenge & Co., Bankers, Pragerstrasse, Dresden, Germany (35). 1887. **H**

Nutting, Prof. Charles C., State University of Iowa, Iowa City, Iowa (40). 1892. **F**

Oberholser, Harry Church, Division Biological Survey, Dep't Agric., Washington, D. C. (46). 1898. **F E H**

Ogden, Herbert G., U. S. C. and G. Survey, Washington, D. C. (38). 1891. **E**

Ordway, Prof. John M., Tulane University, New Orleans, La. (9). 1875. **C**

Orleman, Miss Daisy M., M.D., Peekskill Military Acad., Peekskill, N. Y. (40). 1897. **F**

Orr, William, Jr., 30 Firglade Ave., Springfield, Mass. (39). 1895. **F B**

Orton, Prof. Edward, Ohio State University, Columbus, Ohio (19). 1875. **E**

Osborn, Henry F., Columbia University, New York, N. Y. (29). 1883. **F**

Osborn, Herbert, Ohio State University, Columbus, Ohio (32). 1884. **F**

Osmond, Prof. I. Thornton, State College, Center Co., Pa. (33). 1889. **B A C**

Packard, Dr. A. S., 115 Angell St., Providence, R. I. (16). 1875. **F E**

Paine, Cyrus F., 806 Granite Building, Rochester, N. Y. (12). 1874. **B A**

Palache, Charles, University Museum, Cambridge, Mass. (44). 1896. **E**

Palmer, Prof. Arthur William, Urbana, Ill. (46). 1898. **C**

Pammel, Prof. L. H., Iowa Agricultural College, Ames, Iowa (39). 1892. **G**

PARKHURST, HENRY M., 173 Gates Ave., Brooklyn, N. Y. (23). 1874.
A

Parks, C. Wellman, Civil Eng., U. S. N., care Bureau of Navigation.
Washington, D. C. (42). 1897.

Parsons, Prof. C. Lathrop, Durham, N. H. (41). 1896.

Patterson, Geo. W., Jr., 814 S. University Ave., Ann Arbor, Mich. (44).
1896.

Patterson, Harry J., College Park, Md. (36). 1890. C

Paul, Prof. Henry M., U. S. Naval Observatory, Washington, D. C. (33).
1885. A B

Peet, Rev. Stephen D., 5327 Madison Ave., Chicago, Ill. (24). 1881.
H

Peirce, Prof. Benjamin O., 51 Oxford St., Cambridge, Mass. (47). 1898.

Peirce, George James, Stanford University, Cal. (44). 1897. G

PENROSE, DR. R. A. F., Jr., Ph.D., 460 Bullitt Building, Philadelphia,
Pa. (38). 1890. E

Perkins, Prof. George H., Burlington, Vt. (17). 1882. H F E

Perry, Arthur C., 226 Halsey St., Brooklyn, N. Y. (43). 1896. A B

Peters, Edw. T., P. O. Box 265, Washington, D. C. (33). 1889. I

Pettee, Prof. Wm. H., 554 Thompson St., Ann Arbor, Mich. (24). 1875.
E

Phillips, Prof. A. W., New Haven, Conn. (24). 1879.

Phillips, Prof. Francis C., Western University, Allegheny, Pa. (36). 1889.
C

Phillips, Dr. Wm. A., Evanston, Ill. (41). 1895. H

Pickering, Prof. E. C., Director of Harvard Observatory, Cambridge,
Mass. (18). 1875. A B

Pierce, Perry Benjamin, U. S. Patent Office, Washington, D. C. (40).
1895. H

Pillsbury, J. E., Lieut. U. S. N., care Dr. W. L. Richardson, 225 Com-
monwealth Ave., Boston, Mass. (33). 1898. E B

Pillsbury, Prof. John H., Waban, Mass. (23). 1885. F H

Pinchot, Gifford, U. S. Dep't Agric., Washington, D. C. (47). 1899. G

Platt, Franklin, Ass't Geologist, 2nd Geological Survey of Pennsylvania,
1820 Chestnut St., Philadelphia, Pa. (27). 1882. E

Pohlman, Dr. Julius, Buffalo, N. Y. (32). 1884. E F

Pollard, Charles Louis, 1854 5th St., N. W., Washington, D. C. (44).
1899. G

Porter, Prof. James Madison, Lafayette College, Easton, Pa. (45). 1898. D

Powell, Major J. W., Washington, D. C. (23). 1875. E H

Power, Frederick B., Ph.D., care Messrs. Burroughs, Wellcome & Co., 42
Snow Hill, London, E. C., England (31). 1887. C

Prentiss, D. Webster, M.D., 1101 14th St., N. W., Washington, D. C. (29).
1882. F

Prescott, Prof. Albert B., Ann Arbor, Mich. (23). 1875. C

Pritchett, Henry S., Sup't U. S. C. and G. Survey, Washington, D. C.
(29). 1881. A

Proctor, Miss Mary, 29 E. 46th St., New York, N. Y. (43). 1898. **A**
Prosser, Charles S., Ohio State Univ., Columbus, Ohio (33). 1891. **E F**
Pulsifer, Wm. H., Newton Centre, Mass. (26). 1879. **A H**
Pupin, Dr. M. I., Columbia University, New York, N. Y. (44). 1896. **B**
Putnam, Prof. F. W., Peabody Museum, Cambridge, Mass. (10). 1874. **H**
Ramaley, Francis, Univ. of Colorado, Boulder, Colo. (45). 1899. **G**
Rathbun, Richard, Smithsonian Institution, Washington, D. C. (40).
    1892. **F**
Raymond, Rossiter W., 13 Burling Slip, New York, N. Y. (15). 1875. **E I**
Rees, Prof. John K., Columbia Univ., New York, N. Y. (26). 1878. **A E B**
Reese, Charles L., 1801 Linden Ave., Baltimore, Md. (39). 1892. **C**
Reese, Jacob, 400 Chestnut St., Philadelphia, Pa. (33). 1891. **D B**
Reid, Harry Fielding, Johns Hopkins Univ., Baltimore, Md. (36). 1893. **B**
Remsen, Prof. Ira, Johns Hopkins Univ., Baltimore, Md. (22). 1875. **C**
Reuter, Dr. Ludwig, Merck & Co., Merck Building, University Place,
    New York, N. Y. (46). 1898. **C**
Rice, Prof. Wm. North, Wesleyan University, Middletown, Conn. (18).
    1874. **E F**
Richards, Prof. Charles B., 137 Edwards St., New Haven, Conn. (33).
    1885. **D**
RICHARDS, EDGAR, 341 W. 88th St., New York, N. Y. (31). 1886. **C**
Richards, Prof. Robert H., Massachusetts Institute of Technology, Bos-
    ton, Mass. (22). 1875. **D**
Richards, Mrs. Robert H., Massachusetts Institute of Technology, Bos-
    ton, Mass. (23). 1878. **C**
Richards, Prof. Theodore William, Harvard Univ., Cambridge, Mass.
    (47). 1899.
Richardson, Clifford, Barber Asphalt Paving Co., Long Island City, N. Y.
    (30). 1884. **C**
Ricketts, Prof. Palmer C., 17 1st St., Troy, N. Y. (33). 1887. **D A**
Ricketts, Prof. Pierre de Peyster, 104 John St., New York, N. Y. (26).
    1880. **C D E**
Ries, Heinrich, Ph.B., Columbia University, New York, N. Y. (41). 1898.
    E
Ripley, William Z., Ph.D., Newton Centre, Mass. (44). 1897. **H I**
Robinson, Benjamin Lincoln, Curator Harvard Herbarium, Cambridge,
    Mass. (41). 1893. **G**
Robinson, Prof. Franklin C., Bowdoin College, Brunswick, Me. (29).
    1889. **C D**
Robinson, Prof. S. W., 1353 Highland St., Columbus, Ohio (30). 1883.
    D B A
Rockwell, Gen. Alfred P., Manchester, Mass. (10). 1882. **E**
Rockwell, Chas. H., Box 293, Tarrytown, N. Y. (28). 1883. **A D**
Rockwood, Prof. Charles G., Jr., Princeton College, Princeton, N. J.
    (20). 1874. **A E B D**
Rolfs, P. H., Clemson College P. O., S. C. (41). 1899. **G**
Rominger, Dr. Carl, Ann Arbor, Mich. (21). 1879. **E**

Rood, Prof. O. N., Columbia University, New York, N. Y. (14). 1875. B

Rosa, Edward Bennett, Wesleyan University, Middletown, Conn, (39). 1892. A B

Ross, Waldo O., 1 Chestnut St., Boston, Mass. (29). 1882.

Rotch, A. Lawrence, Director of Blue Hill Meteorological Observatory, Hyde Park, Mass. (39). 1896. B

Rowland, Prof. Henry A., Baltimore, Md. (29). 1880. B

Rowlee, W. W., Cornell University, Ithaca, N. Y. (41). 1894. G

Runkle, Prof. J. D., Massachusets Institute of Technology, Boston, Mass. (2). 1875. A D

Rusby, Henry H., M.D., College of Pharmacy, 211 E. 23d St., New York, N. Y. (36.) 1890. G

Russell, Frank, 25 Ware St., Cambridge, Mass. (45). 1897. H

Russell Prof. H. L., University of Wisconsin, Madison, Wis. (41). 1894. G

Russell, I. C., University of Mich., Ann Arbor, Mich. (25). 1882. E

Ryan, Harris J., Cornell University, Ithaca, N. Y. (38). 1890. B

Safford, Dr. James M., Nashville, Tenn. (6). 1875. E C F

Safford, Prof. Truman H., Williamstown, Mass. (41). 1892. A

Salisbury, Prof. R. D., Chicago University, Chicago, Ill. (37). 1890. B E

Sargent, Dudley Allen, M.D., Director of the Hemenway Gymnasium, Harvard University, Cambridge, Mass. (47). 1899. H

Saunders, Charles E., Ph.D., Experimental Farm, Ottawa, Can. (41). 1895. C

Saunders, William, LL.D., F.R.S.C., F.L.S., Canadian Experimental Farms, Ottawa, Can. (17). 1874. F

Saville, Marshall H., Amer. Mus. Nat. History, Central Park, New York, N. Y. (39). 1892. H

SCHAEBERLE, J. M., Lick Observatory, San José, Cal. (34). 1886. A

Schaffner, John H., Ohio State Univ., Columbus, Ohio (48). 1899. G

Schlotterbeck, Julius O., Ann Arbor, Mich. (46). 1899. G

Schott, Charles A., U. S. C. and G. Survey, Washington, D. C. (8). 1874. A

Schwarz, E. A., 230 New Jersey Ave., Washington, D. C. (29). 1895. F

Schweinitz, Dr. E. A. de, U. S. Depart. Agric., Washington, D. C. (36). 1889. C

Schweitzer, Prof. Paul, State University of Missouri, Columbia, Mo. (24). 1877. C B

SCUDDER, SAMUEL H., Cambridge, Mass. (13). 1874. F

Scull, Miss S. A., Smethport, McKean Co., Pa. (40). 1895. H

Seaman, W. H., Chemist, 1424 11th St., N. W., Washington, D. C. (23). 1874. C F

Sedgwick, Prof. William Thompson, Mass. Institute Technology, Boston, Mass. (47). 1898. F G

See, Horace, 1 Broadway, New York, N. Y. (34). 1886. D

Seymour, Arthur Bliss, Cambridge, Mass. (36). 1890. G

Seymour, Paul Henry, 479 Second Ave., Detroit, Mich. (44). 1896. C
Sharp, Dr. Clayton H., Ithaca, N. Y. (45). 1897. .
Sharples, Stephen P., 13 Broad St., Boston, Mass. (29). 1884. C
Shaw, Prof. James Byrnie, 1030 Grove St., Jacksonville, Ill. (43). 1896. A
Sheldon, Samuel, A.M., Ph.D., Polytechnic Institute, Brooklyn, N. Y.
    (42). 1894. B
Shimer, Porter W., E.M., Ph.D., Easton, Pa. (38). 1889. C
Shufeldt, Dr. R. W., Takoma Park, D. C. (40). 1892. F
Shutt, Frank T., M.A., F.E.C., F.C.S., Canadian Experimental Farm,
    Ottawa, Can. (47). 1898. C
Sias, Solomon, M.D., Schoharie, Schoharie Co., N. Y. (10). 1874.
Simon, Dr. Wm., 1348 Block St., Baltimore, Md. (29). 1895. C
Simonds, Prof. Frederic W., University of Texas, Austin, Texas. (25).
    1888. E F
Skinner, Aaron N., U. S. Naval Observatory, Washington, D. C. (40).
    1893. A
Smith, Alex., Ph.D., University of Chicago, Chicago, Ill. (40). 1892. C
Smith, Prof. Chas. J., 35 Adelbert St., Cleveland, Ohio (32). 1885. A B
Smith, Prof. Edgar F., University of Pennsylvania, Philadelphia, Pa.
    (33). 1891. C
Smith, Prof. Erastus G., Beloit College, Beloit, Wis. (34). 1887. C
Smith, Ernest Ellsworth, 262 Fifth Ave., New York, N. Y. (43). 1898.
Smith, Erwin F., Depart. Agric., Washington, D. C. (34). 1890. G
Smith, Prof. Eugene A., University, Ala. (20). 1877. E C
Smith, Harlan I., Amer. Mus. Nat. History, Central Park, New York,
    N. Y. (41). 1896. H
Smith, Prof. Harold B., Polytechnic Inst., Worcester, Mass. (43). 1898.
    D
Smith, John B., Rutgers College, New Brunswick, N. J. (32). 1884. F
Smith, Quintius C., M.D., 617 Colorado St., Austin, Texas. (26). 1881.
    F
Smock, Prof. John Conover, Trenton, N. J. (23). 1879. E
Smyth, C. H., Jr., Clinton, N. Y. (38). 1894. E
Snow, Prof. F. H., Lawrence, Kan. (29.) 1881. F E
Snyder, Prof. Harry, 2090 Dooley Ave., Saint Anthony Park, Minn. (44).
    1897. C
Snyder, Prof. Monroe B., High School Observatory, Philadelphia, Pa.
    (24). 1882. A B
Soule, R. H., Baldwin Locomotive Works, 1217 Monadnock Building,
    Chicago, Ill. (33). 1886. D
Soule, Wm., Ph.D., Mt. Union College, Alliance, Ohio (33). 1899. B C E
Spencer, Prof. J. William, 1710 Q St., Washington, D. C. (28). 1882.
    E
Spenzer, John G., M.D., 370 Central Avenue, Cleveland, Ohio. (37).
    1895. C
Spinney, L. B., Iowa State College, Ames, Iowa. (42). 1897. B
Springer, Dr. Alfred, Box 621, Cincinnati, Ohio. (24). 1880. C

Squibb, Edward R., M.D., 152 Columbia Heights, Brooklyn, N. Y. (43). 1896.

Starr, Frederick, Ph.D., Prof. University of Chicago, Chicago, Ill. (36). 1892. **H E**

Stearns, Robert E. C., Ph.D., 1025 East Eighteenth St., Los Angeles, Cal. (18). 1874. **F**

Steinmetz, Chas. Proteus, General Electric Co., Schenectady, N. Y. (40). 1895. **B**

STEPHENS, W. HUDSON, Lowville, N. Y. (18). 1874. **E H**

Sternberg, George M., M.D., LL.D., Surgeon-General U. S. A., War Department, Washington, D. C. (24). 1880. **F**

Stevens, Prof. W. LeConte, Washington and Lee Univ., Lexington, Va. (29). 1882. **B**

Stevenson, Mrs. Cornelius, 237 S. 21st St., Philadelphia, Pa. (33). 1895. **H**

Stevenson, Prof. John J., Univ. Heights, New York, N. Y. (36). 1888. **E**

Stevenson, Mrs. Matilda C., Bureau of Ethnology, Washington, D. C. (41). 1893. **H**

Stockwell, John N., 1008 Case Ave., Cleveland, Ohio (18). 1875. **A**

Stokes, Henry Newlin, Ph.D., U. S. Geological Survey, Washington, D. C. (38). 1891. **C E**

Stone, Ormond, Director Leander McCormick Observatory, University of Virginia, Va. (24). 1876. **A**

Story, Prof. Wm. E., Clark University, Worcester, Mass. (29). 1881. **A**

Stowell, Prof. T. B., Potsdam, N. Y. (28). 1885. **F**

Strong, Wendell M., 307 Welch Hall, New Haven, Conn. (44). 1899. **A B**

Sturgis, Wm. C., 384 Whitney Ave., New Haven, Conn. (40). 1892. **G**

Swingle, Walter T., U. S. Depart. Agric., Washington, D. C. (40). 1892. **G**

Tainter, Charles Sumner, 1843 S St., N. W., Washington, D. C. (29). 1881. **B D A**

Talbot, Henry P., Massachusetts Institute of Technology, Boston, Mass. (44). 1896. **C**

TANNER, PROF. JOHN HENRY, 7 Central St., Ithaca, N. Y. (47). 1899. **A B**

Taylor, Frank B., 391 Fairfield Ave., Fort Wayne, Ind. (39). 1897.

Tesla, Nikola, LL.D., 55 W. 27th St., New York, N. Y. (43). 1895. **B**

Thomas, Benjamin F., State University, Columbus, Ohio. (29). 1882. **B A**

Thomas, Prof. M. B., Crawfordsville, Ind. (41). 1894. **G**

Thompson, Joseph Osgood, Amherst, Mass. (41). 1893.

Thomson, Elihu, Swampscott, Mass. (37). 1888. **B**

Thomson, Wm., M.D., 1426 Walnut St., Philadelphia, Pa. (33). 1885. **B**

Thornburg, Charles L., Lehigh University, S. Bethlehem, Pa. (44). 1897. **A**

Thruston, Gates Phillips, Nashville, Tenn. (38). 1890. **H**

Thruston, R. C. Ballard, care Ballard & Ballard Co., Louisville, Ky. (36). 1896. **E**

Thurston, Prof. R. H., Sibley College, Cornell University, Ithaca, N. Y. (23). 1875. D

Tittmann, Otto H., U. S. Coast and Geodetic Survey, Washington, D. C. (24). 1888. A

Todd, Prof. David P., Director Lawrence Observatory, Amherst College, Amherst, Mass. (27). 1881. A B D

Todd, Prof. James E., Box 22, Vermilion, S. Dak. (22). 1886. E F

Tooker, William Wallace, Sag Harbor, N. Y. (43). 1895. H

Traphagen, Frank W., Ph.D., Bozeman, Montana. (35). 1889. C F E

Trelease, Dr. Wm., Director Missouri Botanical Gardens, St. Louis, Mo. (39). 1891. G

Trenholm, Hon. W. L., President American Surety Co., 160 Broadway, New York, N. Y. (35). 1896.

True, Fred W., National Museum, Washington, D. C. (28). 1882. F

Tucker, Willis G., M.D., Albany Medical College, Albany, N. Y. (29). 1888. C

TUCKERMAN, ALFRED, Ph.D., 342 W. 57th St., New York, N. Y. (39). 1891. C

Tuttle, Prof. Albert H., University of Virginia, Charlottesville, Va. (17). 1874. F

Twitchell, E., 720 Greenwood Ave., Avondale, Cincinnati, Ohio (39). 1891. C

Uhler, Philip R., 254 W. Hoffman St., Baltimore, Md. (19). 1874. F E

Underwood, Lucien M., Columbia University, New York, N. Y. (33). 1885. G

Upham, Warren, Minnesota Historical Society, St. Paul, Minn. (25). 1880. E

Upton, Winslow, Brown University, Providence, R. I. (29). 1883. A

Van Dyck, Prof. Francis Cuyler, New Brunswick, N. J. (28). 1882. B C F

Van Hise, Charles R., University of Wisconsin, Madison, Wis. (37). 1890.

Van Vleck, Prof. John M., Wesleyan University, Middletown, Conn. (23). 1875. A

Veeder, Major Albert, M.D., Lyons, Wayne Co., N. Y. (36). 1895.

Venable, Prof. F. P., Chapel Hill, N. C. (39). 1891. C

Verrill, Prof. A. E., New Haven, Conn. (47). 1898. F

Vogdes, Capt. A. W., 5th Artillery, San Juan, Porto Rico, W. I. (32). 1885. E F

Voorhees, Louis A., P. O. Box 290, New Brunswick, N. J. (43). 1895. C

Wagner, Frank C., Rose Polytechnic Institute, Terre Haute, Ind. (34). 1897. D

Waite, M. B.,Dep't of Agriculture, Washington, D. C. (37). 1893. G

Walcott, Charles D., Director U. S. Geological Survey, Washington, D. C. (25). 1882. E F

Waldo, Prof. Clarence A., Purdue University, Lafayette, Ind. (37). 1889. A

Waldo, Leonard, S. D., 57 Coleman St., Bridgeport, Conn. (28). 1880. A

WALLER, E., School of Mines, Columbia University, New York, N. Y. (23). 1874.

Ward, Prof. Henry A., 620 Division St., Chicago, Ill. (13). 1875. **F E H**

Ward, Prof. Henry B., Univ. of Nebraska, Lincoln, Nebr. (48). 1899. **F**

Ward, Lester F., U. S. Geological Survey, Washington, D. C. (26). 1879. **E G**

Ward, Dr. R. H., 53 Fourth St., Troy, N. Y. (17). 1874. **G F**

Ward, Wm. E., Port Chester, N. Y. (36). 1889. **D**

Warder, Prof. Robert B., Howard University, Washington, D. C. (19). 1881. **C B**

WARNER, JAMES D., 463 E. 26th St., Flatbush, Brooklyn, N. Y. (18). 1874. **A B**

Warner, Worcester R., 1722 Euclid Ave., Cleveland, Ohio (33). 1888. **A B D**

Warren, Dr. Joseph W., Bryn Mawr Coll., Bryn Mawr, Pa. (31). 1886. **F**

Warren, Prof. S. Edward, Newton, Mass. (17). 1875. **A I**

Washington, Dr. Henry S., Locust, N. J. (44). 1897. **E**

WATSON, PROF. WM., 107 Marlborough St., Boston, Mass. (12). 1884. **A**

Wead, Charles K., U.S. Patent Office, Washington, D. C. (47). 1898. **B**

Weber, Prof. Henry A., Ohio State Univ., Columbus, Ohio (35). 1888. **C**

Webster, F. M., Wooster, Ohio (35). 1890. **F**

Webster, Prof. N. B., Vineland, N. J. (7). 1874. **B C E**

Weed, Clarence M., Durham, N. H. (38). 1890. **F**

WEST, DR. CHARLES E., Brooklyn, N. Y. (1). 1895.

Weston, Edward, 645 High St., Newark, N. J. (33). 1887. **B C D**

White, David, U. S. Geological Survey, Washington, D. C. (40). 1892. **E F**

White, Prof. H. C., University of Georgia, Athens, Ga. (29). 1885. **C**

WHITE, PROF. I. C., State Geologist of West Virginia, Morgantown, W. Va. (25). 1882. **E**

Whiteaves, J. F., Geological Survey, Ottawa, Can. (31). 1887. **E F**

Whitfield, J. Edward, 406 Locust St., Philadelphia, Pa. (44). 1896. **C**

Whitfield, R. P., Amer. Mus. Nat. History, Central Park, New York, N. Y. (18). 1874. **E F H**

Whiting, Miss Sarah F., Wellesley College, Wellesley, Mass. (31). 1883. **B A**

Whitman, Prof. Charles O., Chicago University, Chicago, Ill. (43). 1898. **F**

Whitman, Prof. Frank P., Adelbert College, Cleveland, Ohio (33). 1885. **B A**

Wilbur, A. B., Middletown, N. Y. (23). 1874. **E**

Wiley, Prof. Harvey W., U. S. Depart. Agric., Washington, D. C. (21). 1874. **C**

Williams, Benezette, 171 La Salle St., Chicago, Ill. (33). 1887. **D**

Williams, Charles H., M.D., 15 Arlington St., Boston, Mass. (22). 1874.

Williams, Prof. Edw. H., Jr., 117 Church St., Bethlehem, Pa. (25). 1894. **E D**

Williams, Francis H., M.D., 505 Beacon St., Boston, Mass. (29). 1890.

Williams, Prof. Henry Shaler, Yale University, New Haven, Conn. (18). 1882. E F

Williams, Prof. Thomas A., U. S. Dep't Agric., Washington, D. C. (42). 1894. G

Willis, Bailey, U. S. Geological Survey, Washington, D. C. (36). 1890.

Willoughby, Charles C., Peabody Museum, Cambridge, Mass. (45). 1897. H

Willson, Prof. Frederick N., Princeton, N. J. (33). 1887. A D

Willson, Robert W., Cambridge, Mass. (30). 1890. B A

Wilson, Joseph M., Room 1030, Drexel Building, Philadelphia, Pa. (33). 1886. D

Wilson, Robert N., Macleod, Alberta, Can. (42). 1895. H

Wilson, Thomas, National Museum, Washington, D. C. (36). 1888. H

Wilson, Prof. William Powell, Director, Philadelphia Museums, 233 S. Fourth St., Philadelphia, Pa. (38). 1889. G

Winchell, Horace V., Butte, Montana. (34). 1890. E C

Winchell, Prof. N. H., University of Minnesota, Minneapolis, Minn. (19). 1874. E H

Winterhalter, A. G., Lt. U. S. N., League Island, Navy Yard, Philadelphia, Pa. (37). 1893. A

Withers, Prof. W. A., Agricultural and Mechanical College, Raleigh, N. C. (33). 1891. C

Witthaus, Dr. R. A., 414 East 26th St., New York, N. Y. (35). 1890.

Wolff, Dr. J. E., 15 Story St., Cambridge, Mass. (36). 1894. E

Woll, Fritz Wilhelm, Madison, Wis. (42). 1897. C

Woodbury, C. J. H., Amer. Bell Telephone Co., 125 Milk St., Boston, Mass. (29). 1884. D

Woodhull, John Francis, Teachers' College, Morningside Heights, New York, N. Y. (43). 1899.

Woodman, Dr. Durand, 80 Beaver St., New York, N. Y. (41). 1896.

Woods, Albert F., Dep't Agric , Washington, D. C. (43). 1897. G

Woodward, Prof. Calvin M., Washington Univ., St. Louis, Mo. (32). 1884. D A I

Woodward, R. S., Columbia University, New York, N. Y. (33). 1885. A B D

Woodworth, William McMichael, Ph.D., 149 Brattle St., Cambridge, Mass. (44). 1898. F

Wright, Prof. Albert A., Oberlin College, Oberlin, Ohio. (24). 1880. E F

Wright, Prof. Arthur W., Yale University, New Haven, Conn. (14). 1874. A B

Wright, Carroll D., LL.D., Department of Labor, Washington, D. C. 1894. I

Wright, Rev. Geo. F., Oberlin College, Oberlin, Ohio. (29). 1882. E H

Würtele, Rev. Louis C., Acton Vale, P. Q. Can. (11). 1875. E

Youmans, Wm. Jay, M.D., Popular Science Monthly, 72 Fifth Avenue, New York, N. Y. (28). 1889. F C

Young, A. V. E., Northwestern University, Evanston, Ill. (33). 1886.
   C B
Young, C. A., Princeton University, Princeton, N. J. (18). 1874. **A B D**
Zalinski, E. L., U. S. A., Century Club, 7 W. 43d St., New York, N. Y.
   (36). 1891. **D**
Ziwet, Alexander, 644 S. Ingalls St., Ann Arbor, Mich. (38). 1890. **A**

[796 HONORARY FELLOWS AND FELLOWS.]

Summary : Patrons, 2 ; Members, 897 ; Honorary Fellows, 2 ; Fellows,
794.

Oct. 30, 1899.   Total number of Members of the Association, 1695.

NOTE.—The omission of an address in the foregoing lists indicates that letters
mailed to that last printed were returned as uncalled for. Information of the present
address of the members so indicated is requested by the PERMANENT SECRETARY.

DECEASED MEMBERS.

[A list of deceased members of the Association, so far as known at the time of publishing the volume of Proceedings of the Springfield meeting, May, 1896, is given in that volume. At the Buffalo meeting the Council directed the Permanent Secretary to omit the printing of the full list of deceased members in the annual volumes and to print only the additions to the list. Since the publication of the list printed in the Boston volume (47) notices have been received of the decease of the following members.]

Bardwell, Elizabeth M., South Hadley, Mass. (46). Died May 27, 1899.

Brinton, D. G., Media, Pa. (33). Born in West Chester, Pa., in 1837. Died in Atlantic City, N. J., July 31, 1899.

Clarke, Robert, Glendale, Ohio. (30). Born in Annan, Dumfriesshire, Scotland, May 1, 1829. Died in Glendale, Ohio, Aug. 26, 1899.

Daly, Charles P., New York, N. Y. (36).

Dana, James J., Washington, D. C. (40).

Emery, Charles E., New York, N. Y. (34). Died June 1, 1898.

Greve, Theodore Lund August, Cincinnati, Ohio (30). Born at St. Michael's Donn, in Dithmarschen, Holstein, April 2, 1830. Died in Cincinnati, Ohio, Dec. 9, 1898.

Hicks, Gilbert H., Washington, D. C. (43). Died Dec. 5, 1898.

Hotchkiss, Jedadiah, Staunton, Va. (31). Born in Windsor, N. Y., Nov. 30, 1828. Died in Staunton, Va., Jan. 17, 1899.

Hubbard, Henry Guernsey, Washington, D. C. (41). Died in Crescent City, Fla., Jan. 18, 1899.

Hunt, Alfred E., Pittsburg, Pa. (35). Died April 26, 1899.

Jillson, B. C., Pittsburg, Pa. (14). Died July 19, 1899.

Kendall, E. Otis, Philadelphia, Pa. (29). Died Jan. 6, 1899.

Marcy, Oliver, Evanston, Ill. (10). Died in Evanston, Ill., March 19, 1899.

Marsh, Othniel Charles, New Haven, Conn. (15). Born in Lockport, N. Y., Oct. 29, 1831. Died in New Haven, Conn., March 18, 1899.

Pike, J. W., Mahoning, Ohio (29). Born in Concord, Ohio, June 27, 1828. Died in Washington, D. C., Dec. 21, 1898.

Schanck, J. Stillwell, Princeton, N. J. (4). Died Dec. 16, 1898.

Smith, Benjamin G., Cambridge, Mass. (29).

Stam, Colin F., Chestertown, Md. (33). Died Aug. 22, 1898.

Stowell, John, Charlestown, Mass. (21).

Stuart, Ambrose P. S., Lincoln, Neb. (21). Born in Sterling, Mass., Nov. 22, 1820. Died in Lincoln, Neb., Sept. 14, 1899.

Sturtevant, E. Lewis, South Framingham, Mass. (29). Died July 31, 1898.

Thompson, Elizabeth, Stamford, Conn. (22). Died in Stamford, Conn., July, 1899.

Thompson, Mrs. Frank, Philadelphia, Pa. (33).

Townsend, Franklin, Albany, N. Y. (4). Died Sept. 1898.

Trimble, Henry, Philadelphia, Pa. (34). Died Aug. 24, 1898.

Vermyné, J. J. B., New Bedford, Mass. (29). Born in Zutphen, Holland, in 1835. Died in Francestown, N. H., Aug. 16, 1898.

Weeden, Joseph E., Randolph, N. Y. (31). Died Jan. 31, 1899, aged 89 years.

While engaged in writing the address that I am to read to you this evening, the sad news reached me of the death, on July 31, of our President of five years ago, Doctor D. G. Brinton. Although not unexpected, as his health had been failing since he was with us at the Boston meeting where he took his always active part in the proceedings of Section H, and gave his wise advice in our general council, yet his death affects me deeply. I was writing on a subject we had often discussed in an earnest but friendly manner. He believed in an all-pervading psychological influence upon man's development, and claimed that American art and culture were autochthonous, and that all resemblances to other parts of the world were the results of corresponding stages in the development of man ; while I claimed that there were too many root coincidences with variant branches to be fully accounted for without also admitting the contact of peoples. Feeling his influence while writing, I had hoped that he would be present to-night, for I am certain that no one would have more readily joined with me in urging a suspension of judgment, while giving free expression to opinions, until the facts have been worked over anew, and more knowledge attained.

His eloquent tongue is silent and his gifted pen is still, but I urge upon all who hear me to-night to read his two addresses before this Association : one as Vice-President of the Anthropological section in 1887, published in our 36th volume of Proceedings, the other as Retiring President in 1895, published in our 44th volume. In these addresses he has in his usual forceable and comprehensive manner presented his views of American anthropological research and of the aims of anthropology·

Dr. Brinton was a man of great mental power and erudition. He was an extensive reader in many languages, and his retentive memory enabled him to quote readily from the works of others. He was a prolific writer, and an able critic of anthropological literature the world over. Doing little as a field archæologist himself, he kept informed of what was done by others through extensive travels and visits to museums. By his death American anthropology has suffered a serious loss, and a great scholar and earnest worker has been taken from our Association.

<div align="right">F. W. P.</div>

ADDRESS

BY

FREDERIC WARD PUTNAM,

THE RETIRING PRESIDENT OF THE ASSOCIATION.

A PROBLEM IN AMERICAN ANTHROPOLOGY.

In the year 1857 this Association met for the first time beyond the borders of the United States, thus establishing its claim to the name American in the broadest sense. Already a member of a year's standing, it was with feelings of youthful pride that I recorded my name and entered the meeting in the hospitable city of Montreal ; and it was on this occasion that my mind was awakened to new interests which in after years led me from the study of animals to that of man.

On Sunday, August 16th, while strolling along the side of Mount Royal, I noticed the point of a bivalve shell protruding from the roots of grass. Wondering why such a shell should be there and reaching to pick it up, I noticed, on detaching the grass roots about it, that there were many other whole and broken valves in close proximity — too many, I thought, and too near together to have been brought by birds, and too far away from water to be the remnants of a musk-rat's dinner. Scratching away the grass and poking among the shells, I found a few bones of birds and fishes and small fragments of Indian pottery. Then it dawned upon me that here had been an Indian home in ancient times and that these odds and ends were the refuse of the people — my first shell-heap or kitchen-midden, as I was to learn later. At the time, this was to me simply the evidence of Indian occupation of the place in former times, as convincing as was the palisaded town

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of old Hochelaga to Cartier when he stood upon this same mountain side more than three centuries before.

At that meeting of the Association several papers were read, which, had there been a Section of Anthropology, would have led to discussions similar to those that have occurred during our recent meetings. Forty-two years later we are still disputing the evidence, furnished by craniology, by social institutions and by language, in relation to the unity or diversity of the existing American tribes and their predecessors on this continent.

Those were the days when the theory of the unity of all American peoples, except the Eskimo, as set forth by Morton in his 'Crania Americana' (1839), was discussed by naturalists. The volumes by Nott and Gliddon, 'Types of Mankind' (1854) and 'Indigenous Races of the Earth' (1857), which contains Meigs' learned and instructive dissertation, 'The Cranial Characteristics of the Races of Men,' were the works that stirred equally the minds of naturalists and of theologians regarding the unity or diversity of man—a question that could not then be discussed with the equanimity with which it is now approached. The storm caused by Darwin's 'Origin of Species' had not yet come to wash away old prejudices and clear the air for the calm discussion of theories and facts now permitted to all investigators. Well do I remember when, during those stormy years, a most worthy Bishop made a fervent appeal to his people to refrain from attending a meeting of the Association, then being held in his city, on account of what he claimed to be the atheistic teachings of science. Yet ten years later this same venerable Bishop stood before us, in that very city, and invoked God's blessing upon the noble work of the searchers for truth.

At the meeting of 1857, one of our early Presidents, the honored Dana, read his paper, entitled 'Thoughts on Species,' in which he described a species as " a specific amount or condition of concentrated force defined in the act or law of creation," and, applying this principle, determined the unity of man in the following words :

" We have, therefore, reason to believe, from man's fertile intermixture, that he is one in species, and that all organic

species are divine appointments which cannot be obliterated unless by annihilating the individuals representing the species.''

Another paper was by Daniel Wilson, recently from Scotland, where six years before he had coined that most useful word, 'prehistoric,' using the term in the title of his volume, ' Prehistoric Annals of Scotland.' In his paper Professor (afterward Sir Daniel) Wilson controverted the statement of Morton that there was a single form of skull for all American peoples, north and south, always excepting the Eskimo. After referring to the views of Agassiz, as set forth in the volumes of Nott and Gliddon, he said: ''Since the idea of the homogeneous physical characteristics of the whole aboriginal population of America, extending from Terra del Fuego to the Arctic circle, was first propounded by Dr. Morton, it has been accepted without question, and has more recently been made the basis of many widely comprehensive deductions. Philology and archæology have also been called in to sustain this doctrine of a special unity of the American race, and to prove that, notwithstanding some partial deviations from the prevailing standard, the American Indian is essentially separate and peculiar—*a race distinct from all others.* The stronghold, however, of the argument for the essential oneness of the whole tribes and nations of the American continents is the supposed uniformity of physiological and especially of physiognomical and cranial characteristics—an ethical postulate which has not yet been called in question.''

After a detailed discussion of a number of Indian crania from Canada and a comparison with those from other parts of America, as described by Morton, Wilson makes the following statements : '' But making full allowance for such external influences, it seems to me, after thus reviewing the evidence on which the assumed unity of the American race is formed, a little less extravagant to affirm of Europe than of America that the crania everywhere and at all periods have conformed, or even approximated, to one type.

'' As an hypothesis, based on evidence accumulated in the Crania Americana, the supposed homogeneity of the whole American aborigines was perhaps a justifiable one. But the evidence was totally insufficient for any such absolute and

dogmatic induction as it has been made the basis of. With the exception of the ancient Peruvians, the comprehensive generalizations relative to the southern American continent strangely contrast with the narrow basis of the premises. With a greater amount of evidence in reference to the northern continent, the conclusions still go far beyond anything established by absolute proof ; and the subsequent labors of Morton himself, and still more of some of his successors, seem to have been conducted on the principle of applying practically, and in all possible bearings, an established and indisputable scientific truth, instead of testing by further evidence a novel and ingenious hypothesis.''

At the close of this instructive paper are the following words : ''If these conclusions, deduced from an examination of Canadian crania, are borne out by the premises, and confirmed by further investigation, this much at least may be affirmed : that a marked difference distinguishes the northern tribes, now or formerly occupying the Canadian area, in their cranial conformation, from that which pertains to the aborigines of Central America and the southern valley of the Mississippi ; and in so far as the northern differ from the southern tribes they approximate more or less, in the points of divergence, to the characteristics of the Esquimaux : that intermediate ethnic link between the Old and the New World, acknowledged by nearly all recent ethnologists to be physically a Mongol and Asiatic, if philologically an American.''

The third paper of the meeting to which I shall refer was by another of our former Presidents, the then well-known student of Indian institutions and the author of the ' League of the Iroquois ' (1851). In this paper on ' The Laws of Descent of the Iroquois,' Morgan discusses the League as made up of five nations each of which was subdivided into tribes, the family relationship and the descent in the female line as essential to the maintenace of the whole system. He then says :

'' Now the institutions of all the aboriginal races of this continent have a family cast. They bear internal evidence of a common paternity, and point to a common origin, but remote, both as to time and place. That they all sprang from a common mind, and in their progressive development have still re-

tained the impress of original elements, is abundantly verified. The Aztecs were thoroughly and essentially Indian. We have glimpses here and there at original institutions which suggest at once, by their similarity, kindred ones among the Iroquois and other Indian races of the present day. Their intellectual characteristics, and the predominant features of their social condition, are such as to leave no doubt upon this question; and we believe the results of modern research upon this point concur with this conclusion. Differences existed, it is true, but they were not radical. The Aztec civilization simply exhibited a more advanced development of those primary ideas of civil and social life which were common to the whole Indian family, and not their overthrow by the substitution of antagonistic institutions.''

After calling attention to the fact that a similar condition exists among certain peoples of the Pacific Islands, he writes: '' Whether this code of descent came out of Asia or originated upon this continent is one of the questions incapable of proof; and it must rest, for its solution, upon the weight of evidence or upon probable induction. Its existence among American races whose languages are radically different, and without any traditional knowledge among them of its origin, indicates a very ancient introduction; and would seem to point to Asia as the birthplace of the system.''

It would be interesting to follow the succeeding meetings of the Association and note the recurring presentation of views which the quotations I have given show to have been most seriously discussed over a generation ago. An historical review of the literature of American anthropology during the present century would also be interesting in this connection. It is probable, however, that a review of this literature for the first half of the century would reveal the fact that the writers, with here and there a notable exception, were inclined to theorize upon insufficient data and devoted little time to the accumulation of trustworthy facts. The presentation and discussion of carefully observed facts can almost be said to have begun with the second half of the century, and this is the only part of the subject that now commands serious attention.

A reference to the very latest *résumé* of this subject as pre-

sented in the 'History of the New World called America,' by
Edward John Payne, Vol. II., Oxford, 1899, is instructive
here. In this volume Mr. Payne expresses his belief in the
antiquity and unity of the American tribes, which he considers
came from Asia in preglacial and glacial times, when the north-
western corner of America was connected with Asia, and when
man "as yet was distinguished from the inferior animals only
by some painful and strenuous form of articulate speech and
the possession of rude stone weapons and implements, and a
knowledge of the art of fire-kindling. Such, it may be sup-
posed, were the conditions under which man inhabited both
the Old and the New World in the paleo-ethnic age.   *   *
Even when a geological change had separated them [the conti-
nents] some intercourse by sea was perhaps maintained—an
intercourse which became less and less, until the American
branch of humanity became practically an isolated race, as
America itself has become an isolated continent." (Preface.)

Mr. Payne discusses the growth of the languages of
America, the various social institutions and arts, and the migra-
tions of these early savages over the continent, north and
south, during the many centuries following, as one group after
another grew in culture. He considers all culture of the people
autochthonous. "It may, however, be suggested that, as in
the Old World, the earlier and the smaller tribes tend to doli-
chocephaly, while the better developed ones are rather brachy-
cephalous—a conclusion indicating that the varying propor-
tions of the skull should be taken less as original evidence of
race than as evidence of physical improvement."

This volume by Mr. Payne is replete with similar statements
of facts and theories, and shows how difficult it is for us to
understand the complications of the subject before us. It can-
not be denied that, taking into consideration the number of
authors who have written on this subject, Mr. Payne is well
supported in his theory of the autochthonous origin of all
American languages, institutions and arts; but the question
arises: Has not the old theory of Morton, the industrious and
painstaking pioneer of American craniology, been the underly-
ing cause of this, and have not the facts been misinterpreted?
At the time of Morton the accepted belief in the unity and uni-

versal brotherhood of man was about to be assailed, and it seems, as we now look back upon those times of exciting and passionate discussions, that Morton may have been influenced by the new theory which was so soon to become prominent— that there were several distinct creations of species of the genus *Homo* and that each continent or great area had its own distinct fauna and flora. Certainly Morton ventured to make a specific statement from a collection of crania which would now be regarded as too limited to furnish true results.

The anthropologist of to-day would hardly venture to do more than to make the most general statements of the characters of any race or people from the examination of a single skull; although, after the study of a large number of skulls from a single tribe or special locality, he would probably be able to select one that was distinctly characteristic of the special tribe or group to which it pertained.

Relatively long and narrow heads and short and broad heads occur almost everywhere in greater or less proportion. In determining the physical characters of a people, so far as this can be done from a study of crania, the index of the height of the skull is quite as important as that of its breadth. These indices simply give us the ready means of expressing by figures the relative height and breadth of one skull in comparison with another, a small part of what the zoologist would consider in describing, for instance, the skulls of the different species of the genus *Canis*. So in our craniological studies we should determine the relative position, shape and proportions of the different elements of the skull. In fact, we should approach the study of human crania with the methods of the zoologist, and should use tables of figures only so far as such tables give us the means of making exact comparisons. Here, again, are the anthropologists at a disadvantage, inasmuch as it is only very recently that we are approaching a standard of uniformity in these expressions. It is now more than ever essential that the anthropologists should agree upon a method of expressing certain observed facts in somatology, so that the conscientious labors of an investigator who has had a special opportunity for working upon one group of man may be made available for comparison by investigators of other groups.

Probably the old method, still largely in vogue, of stating averages is responsible for many wrong deductions. If we take one hundred or more skulls of any people we shall find that the two extremes of the series differ, to a considerable extent, from those which naturally fall into the center of the series. These extremes in the hands of a zoologist would be considered the subvarieties of the central group or variety. So in anthropology we should take the central group of the series as furnishing the true characters of the particular variety or group of man under consideration, and should regard the extremes as those which have been modified by various causes. It may be said that this central group is defined by stating the mean of all the characters, but this is hardly the case, for by giving the mean of all we include such extraneous characters as may have been derived by admixture or from abnormal conditions.

The many differing characteristics exhibited in a large collection of crania, brought together from various portions of America, north and south, it seems to me, are reducible to several great groups. These may be generally classed as the Eskimo type the northern and central or so-called Indian type, the northwestern brachycephalic type, the southwestern dolichocephalic type, the Toltecan brachycephalic type and the Antillean type, with probably the ancient Brazilian, the Fuegian and the pre-Inca types of South America. Each of these types is found in its purity in a certain limited region, while in other regions it is more or less modified by admixture. Thus the Toltecan, or ancient Mexican, type (which, united with the Peruvian, was separated as the Toltecan family even by Morton) occurs, more or less modified by admixture in the ancient and modern pueblos and in the ancient earth-works of our central and southern valleys. In Peru, more in modern than in ancient times, there is an admixture of two principal types. At the north of the continent we again find certain traits that possibly indicate a mixture of the Eskimo with the early coast peoples both on the Pacific and on the Atlantic sides of the continent. The north-central Indian type seems to have extended across the continent and to have branched in all directions, while a similar but not so extensive branching, north-

east and south, seems to have been the course of the Toltecan type.

This is not theorizing upon the same facts from which Morton drew the conclusion that all these types were really one and the same. Since Morton's time we have had large collections of crania for study, and the crania have been correlated with other parts of the skeleton and with the arts and institutions of the various peoples.

Although these relations have been differently interpreted by many anthropologists who have treated the subject, yet to me they seem to indicate that the American continent has been peopled at different times and from various sources ; that in the great lapse of time since the different immigrants reached the continent there has been in many places an admixture of the several stocks and a modification of the arts and customs of all, while natural environment has had a great influence upon the ethnic development of each group. Furthermore, contact of one group with another has done much to unify certain customs, while 'survivals' have played an active part in the adoption and perpetuation of arts and customs not native to the people by whom they are preserved.

The Inca civilization, a forcible one coming from the north, encroached upon that of the earlier people of the vicinity of Lake Titicaca, whose arts and customs were, to a considerable extent, adopted by the invaders. It is of interest here to note the resemblance of the older Andean art with that of the early Mediterranean, to which it seemingly has a closer resemblance than to any part on the American continent. Can it be that we have here an æsthetic survival among this early people, and could they have come across the Atlantic from that Eurafric region which has been the birthplace of many nations? Or is this simply one of those psychical coincidences, as some writers would have us believe ? The customs and beliefs of the Incas point to a northern origin and have so many resemblances to those of the ancient Mexicans as hardly to admit of a doubt that in early times there was a close relation between these two widely separated centers of ancient American culture. But how did that pre-Inca people reach the lake region? Is it not probable that some phase of this ancient culture may have

reached the Andes from northern Africa? Let us consider this question in relation to the islands of the Atlantic. The Canary Islands, as well as the West Indies, had long been peopled when first known to history ; the Caribs were on the northern coast of South America, as well as on the islands; and in the time of Columbus native trading boats came from Yucatan to Cuba. We thus have evidence of the early navigation of both sides of the Atlantic, and certainly the ocean between could easily have been crossed.

One of the most interesting as well as most puzzling of the many phases of American archæology is the most remarkable development of the art of the brachycephalic peoples, extending from northern Mexico northeastward to the Mississippi and Ohio valleys, then disappearing gradually as we approach the Alleghenies and, farther south, the Atlantic coast, also spreading southward from Mexico to Honduras, and changing and vanishing in South America. Unquestionably of very great antiquity, this art, developed in the neolithic period of culture, reached to the age of metals, and had already begun to decline at the time of the Spanish conquest. How this remarkable development came to exist amid its different environments we cannot yet fully understand ; but the question arises : Was it of autochthonous origin and due to a particular period in man's development, or was it a previously existing phase modified by new environment? For the present this question should be held in abeyance. To declare that the resemblance of this art to both Asiatic and Egyptian art is simply a proof of the psychical unity of man, is assuming too much and is cutting off all further consideration of the subject.

The active field and museum archæologist who knows and maintains the association of specimens as found, and who arranges them in their geographical sequence, becomes intimately in touch with man's work under different phases of existence. Fully realizing that the natural working of the human mind under similar conditions will to a certain extent give uniform results, he has before him so many instances of the transmission of arts, symbolic expressions, customs, beliefs, myths and languages, that he is forced to consider the lines of contact and migration of peoples as well as their psychical resemblances.

It must be admitted that there are important considerations, both physical and mental, that seem to prove a close affinity between the brown type of eastern Asia and the ancient Mexicans. Admitting this affinity, the question arises: Could there have been a migration eastward across the Pacific in neolithic times, or should we look for this brown type as originating in the Eurafric region and passing on to Asia from America? This latter theory cannot be considered as a baseless suggestion when the views of several distinguished anthropologists are given the consideration which is due to them. On the other hand, the theory of an early migration from Asia to America may also be applied to neolithic time.

However this may have been, what interests us more at this time, and in this part of the country, is the so-called ' Mound Builder' of the Ohio valley. Let us first clear away the mist which has so long prevented an understanding of this subject by discarding the term 'Mound Builder.' Many peoples in America, as well as in other continents, have built mounds over their dead, or to mark important sites and great events. It is thus evident that a term so generally applied is of no value as a scientific designation. In North America the term has been applied even to refuse piles; the kitchen-middens or shell-heaps which are so numerous along our coasts and rivers have been classed as the work of the ' Mound Builder.' Many of these shell-heaps are of great antiquity, and we know that they are formed of the refuse gathered on the sites of the early peoples. From the time of these very early deposits to the present such refuse piles have been made, and many of the sites were reoccupied, sometimes even by a different people. These shell-heaps, therefore, cannot be regarded as the work of one people. The same may be said in regard to the mounds of earth and of stone so widely distributed over the country. Many of these are of great antiquity, while others were made within the historic period and even during the first half of the present century. Some mounds cover large collections of human bones ; others are monuments over the graves of noted chiefs ; others are in the form of effigies of animals and of man ; and, in the south, mounds were in use in early historic times as the sites of ceremonial or other important buildings. Thus it will be

seen that the earth-mounds, like the shell-mounds, were made by many peoples and at various times.

There are, however, many groups of earthworks which, although usually classed as mounds, are of an entirely different order of structure and must be considered by themselves. To this class belong the great embankments, often in the form of squares, octagons, ovals and circles, and the fortifications and singular structures on hills and plateaus, which are in marked contrast to the ordinary conical mounds. Such are the Newark, Liberty, Highbank and Marietta groups of earthworks, the Turner group, the Clark or Hopewell group, and many others in Ohio and in the regions generally south and west of these great central settlements; also the Cahokia Mound opposite St. Louis, the Serpent Mound of Adams County, the great embankments known as Fort Ancient, the truly wonderful work of stone known as Fort Hill in Highland County, and the strange and puzzling walls of stone and cinder near Foster's Station, on the Little Miami river.

So far as these older earthworks have been carefully investigated, they have proved to be of very considerable antiquity. This is shown by the formation of a foot or more of vegetable humus upon their steep sides, by the forest growth upon them which is often of primeval character, and by the probability that many of these works, covering hundreds of acres, were planned and built upon the river terraces before the growth of the virgin forest.

If all mounds of shell, earth or stone, fortifications on hills, or places of religious and ceremonial rites, are classed irrespective of their structure, contents, or time of formation, as the work of one people, and that people is designated 'the American Indian' or the 'American Race,' and considered to be the only people ever inhabiting America, north and south, we are simply repeating what was done by Morton in relation to the crania of America—not giving fair consideration to differences while overestimating resemblances. The effort to affirm that all the various peoples of America are of one race has this very year come up anew in the proposition to provide 'a name which shall be brief and expressive' and at the same time shall fasten upon us the theory of unity—notwithstanding the facts show diversity—of race.

Let us now return to the builders of the older earthworks, and consider the possibility of their having been an offshoot of the ancient Mexicans. Of the crania from the most ancient earthworks we as yet know so little that we can only say that their affinities are with the Toltecan type ; but of the character of the art, and particularly the symbolism expressing the religious thought of the people, we can find the meaning only by turning to ancient Mexico. What northern or eastern Indian ever made or can understand the meaning of such sculptures or such incised designs as have been found in several of the ancient ceremonial mounds connected with the great earthworks ? What Indian tribe has ever made similar carved designs on human and other bones, or such singular figures, cut out of copper and mica, as were found in the Turner and Hopewell groups ? Or such symbolic animal forms, elaborately carved in stone, and such perfect terra cotta figures of men and women as were found on the sacrificial altars of the Turner group? What meaning can be given to the Cincinnati Tablet or to the designs on copper plates and shell discs from some of the southern and western burial and ceremonial mounds? I think we shall search in vain for the meaning of these many objects in the north or east, or for much that resembles them in the burial places of those regions. On the other hand, most of these become intelligible when we compare the designs and symbols with those of the ancient Mexican and Central American peoples. The Cincinnati Tablet which has been under discussion for over half a century can be interpreted and its dual serpent characters understood by comparing it with the great double image known in Mexico as the Goddess of Death and the God of War. The elaborately complicated designs on copper plates, on shell discs, on human bones and on the wing bones of the eagle can in many instances be interpreted by comparison with Mexican carvings and with Mexican modes of symbolic expression of sacred objects and religious ideas. The symbolic animals carved on bone or in stone and the perfection of the terra cotta figures point to the same source for the origin of the art.

In connection with the art of the builders let us consider the earth structures themselves. The great mound at Cahokia,

with its several platforms, is only a reduction of its prototype at Cholula. The fortified hills have their counterparts in Mexico. The serpent effigy is the symbolic serpent of Mexico and Central America. The practice of cremation and the existence of altars for ceremonial sacrifices strongly suggest ancient Mexican rites. We must also recall that we have a connecting link in the ancient Pueblos of our own southwest, and that there is some evidence that in our southern states, in comparatively recent times, there were a few remnants of this old people. It seems to me, therefore, that we must regard the culture of the builders of ancient earthworks as one and the same with that of ancient Mexico, although modified by environment.

Our northern and eastern tribes came in contact with this people when they pushed their way southward and westward, and many arts and customs were doubtless adopted by the invaders, as shown by customs still lingering among some of our Indian tribes. It is this absorption and admixture of the peoples that has in the course of thousands of years brought all our American peoples into a certain conformity. This does not, however, prove a unity of race.

It is convenient to group the living tribes by their languages. The existence of more than a hundred and fifty different languages in America, however, does not prove a common origin, but rather a diversity of origin as well as a great antiquity of man in America.

That man was on the American continent in the quaternary times and possibly still earlier, seems to me as certain as that he was on the European during the same period. The Calaveras skull, that bone of contention, is not the only evidence of his early occupation of the Pacific coast. On the Atlantic side the recent extensive explorations of the glacial and immediately following deposits at Trenton are confirmatory of the occupation of the Delaware valley during the closing centuries of the glacial period and possibly also of the interglacial time. The discoveries in Ohio, in Florida, and in various parts of Central and South America, all go to prove man's antiquity in America. Admitting the great antiquity of one or more of the early groups of man on this continent, and that he spread widely

over it while in the palæolithic and early neolithic stages of culture, I cannot see any reason for doubting that there were also later accessions during neolithic times and even when social institutions were well advanced. While these culture epochs mark certain phases in the development of a people, they cannot be considered as marking special periods of time. In America we certainly do not find that correlation with the Old World periods which we are so wont to take for granted.

We have now reached the epoch of careful and thorough exploration and of conscientious arrangement of collections in our scientific museums. It is no longer considered sacrilegious to exhibit skulls, skeletons and mummies in connection with the works of the same peoples. Museums devoted primarily to the education of the public in the æsthetic arts are clearing their cases of heterogeneous collections of ethnological and archæological objects. Museums of natural history are being arranged to show the history and distribution of animal and vegetable life and the structure of the earth itself. Anthropological museums should be similarly arranged and, with certain gaps, which every curator hopes to fill, they should show the life and history of man. To this end the conscientious curator will avoid the expression of special theories and will endeavor to present the true status of each tribe or group of man in the past and in the present, so far as the material at his command permits. A strictly geographical arrangement is, therefore, the primary principle which should govern the exhibition of anthropological collections. A special exhibit may be made in order to illustrate certain methods by which man in different regions has attained similar results, either by contact or by natural means. Another exhibit may be for the purpose of showing the distribution of corresponding implements over different geographical areas. These and similar special exhibits are instructive and under proper restrictions should be made, but unless the design of each exhibit is clearly explained, the average visitor to a museum will be confused and misled, for such objects so grouped convey a different impression than when exhibited with their associated objects in proper geographical sequence.

The anthropology of America is now being investigated and

the results are being made known through museums and publications as never before.

The thoroughly equipped Jesup North Pacific Expedition, with well-trained anthropologists in charge, was organized for the purpose of obtaining material both ethnological and archæological for a comparative study of the peoples of the northern parts of America and Asia. Although only in the third year of its active field work, it has already furnished most important results and provided a mass of invaluable authentic material.

The Hyde Expedition planned for long-continued research in the archæology and ethnology of the southwest, a successor in regard to its objects to the important Hemenway Expedition, is annually adding chapters to the story of the peoples of the ancient Pueblos.

The results of the extensive explorations by Moore of the mounds of the southern Atlantic coast are being published in a series of important monographs.

The Pepper-Hurst Expedition to the Florida Keys has given information of remarkable interest and importance from a rich archæological field before unknown.

The United States government, through the Bureau of Ethnology of the Smithsonian Institution, has given official and liberal support to archæological and ethnological investigations in America.

The constantly increasing patronage, by wealthy men and women, of archæological research at home, as well as in foreign lands, is most encouraging.

The explorations in Mexico and in Central and South America, the publications in fac-simile of the ancient Mexican and Maya codices, the reproduction by casts of the important American sculptures and hieroglyphic tablets, all have been made possible by earnest students and generous patrons of American research.

The numerous expeditions, explorations and publications of the Smithsonian Institution and of Museums of Washington, Chicago, Philadelphia, New York and Cambridge are providing the student of to-day with a vast amount of authentic material for research in American and comparative anthropology.

The Archæological Institute of America, the American Folk-Lore Society and the archæological and anthropological societies and clubs, in active operation in various parts of the country, together with the several journals devoted to different branches of anthropology, give evidence of wide-spread interest.

Universities are establishing special courses in anthropology, and teachers and investigators are being trained. Officers of anthropological museums are preparing men to be field workers and museum assistants. The public need no longer be deceived by accounts of giants and other wonderful discoveries. The wares of the mercenary collector are at a discount since unauthentic material is worthless.

Anthropology is now a well-established science ; and with all this wealth of materials and opportunities, there can be no doubt that in time the anthropologists will be able to solve that problem which for the past half century has been discussed in this Association—the problem of the unity or diversity of prehistoric man in America.

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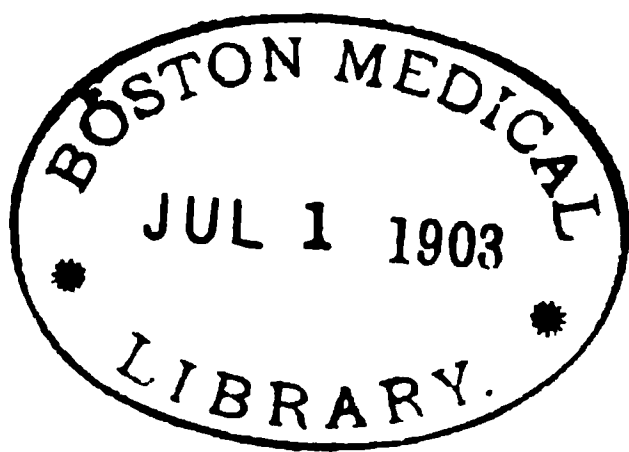
ADDRESS

BY

ALEXANDER MACFARLANE,

VICE-PRESIDENT AND CHAIRMAN OF SECTION A.

THE FUNDAMENTAL PRINCIPLES OF ALGEBRA.

This section of the Association, over which I have the honor of being called upon to preside, may be said to be a double section, for it comprises both mathematics and astronomy; as a consequence, the addresses which have been delivered by my predecessors fall into two distinct groups, the mathematical and the astronomical. Of the former class I have had the pleasure of listening to three: Professor Gibbs on Multiple Algebra, Professor Hyde on the Development of Algebra, and Professor Beman on a Chapter in the History of Mathematics. Each of these addresses was devoted to one feature or other of the development of Algebra, and the subject which I have chosen for to-day is another aspect of the same wonderful phenomenon. It is a subject which interests alike the mathematician and the philosopher, and indeed all thinking men, for it concerns the foundations of that science which is generally acknowledged to be the most perfect creation of the human intellect.

I propose then to review historically and critically the several advances which have been made respecting the fundamental principles of algebra. Here I am mindful of the advice which Horace gives a young poet, not to begin his epic at the origin of things, but to hasten on to the event proper;

consequently, I shall not go back to the Egyptians, Greeks, Hindoos, or Arabs, but at once proceed to the advances made in the present century.

One of the first results of the differential notation of Leibnitz was the recognition of the analogy between $\frac{d}{dx}$ the symbol of differentiation and the ordinary symbol of algebra ; later the same analogy was perceived to hold for Δ_x the symbol of the calculus of finite differences.   Guided by this analogy, Lagrange and other mathematicians of the French school, which flourished at the beginning of the century, inferred that theorems proved to be true for combinations of ordinary symbols of quantity might be applied to the differential calculus and the calculus of finite differences.   In this way many theorems were enunciated, which appeared to be true, but of which it was thought to be almost impossible to obtain direct demonstration. Gradually, however, the view was reached that the logical connection amounted to more than analogy, and that the common theorems were true because the symbols in the three cases were subject to the same fundamental laws of combination.   This advance was principally made by Servais, who enunciated the laws of commutation and distribution.

About the year 1812 a school of mathematicians arose at Cambridge which aimed at introducing the d-ism of the Continent in place of the dot-age of the University ; in other words they believed in the practical superiority of the differential notation of Leibnitz over the fluxional notation of Newton.   Their attention was naturally drawn to the questions which had sprung from the differential notation ; and of the three founders of the school,—Babbage, Herschel, Peacock,— the last named took up the problem of placing the teaching of algebra more in consonance with the views which had been reached of the nature of symbols.   Peacock considered algebra as then taught to be more of an art than a science ; a collection of rules rather than a system of logically connected principles; and with the object of placing it on a more scientific basis, he made a distinction between arithmetical algebra and symbolical algebra.   He treated these names as denoting distinct sciences, and he wrote an algebra in two volumes, of which

one treats of arithmetical algebra and the other of symbolical algebra. He thus describes what he means by the former term: "In arithmetical algebra we consider symbols as representing numbers and the operations to which they are submitted as included in the same definitions as in common arithmetic; the signs $+$ and $-$ denote the operations of addition and subtraction in their ordinary meaning only, and those operations are considered as impossible in all cases where the symbols subjected to them possess values which would render them so in case they were replaced by digital numbers; thus in expressions such as $a + b$ we must suppose a and b to be quantities of the same kind; in others, like $a - b$, we must suppose a greater than b and therefore homogeneous with it; in products and quotients, like $a b$ and $\frac{a}{b}$ we must suppose the multiplier and divisor to be abstract numbers; all results whatsoever, including negative quantities, which are not strictly deducible as legitimate conclusions from the definitions of the several operations must be rejected as impossible, or as foreign to the science."

Here it may be observed that Peacock is not true to his own principle; for $\frac{a}{b}$ is as impossible when b is not a divisor of a, as is $a - b$, when b is not less than a; in neither case do we get a digital number. He draws the line so as to exclude the fraction as a multiplier but not as a multiplicand; according to his own principle it should be excluded from arithmetical algebra. But arithmetic so restricted would be a very narrow science, and the logical result would be to divide arithmetic itself into an arithmetical arithmetic and a symbolical arithmetic.

He thus describes what he means by "symbolical algebra." "Symbolical algebra adopts the rules of arithmetical algebra but removes altogether their restrictions; thus symbolical subtraction differs from the same operation in arithmetical algebra in being possible for all relations of value of the symbols or expressions employed. All the results of arithmetical algebra which are deduced by the application of its rules, and which are general in form, though particular in value, are results

likewise of symbolical algebra where they are general in value as well as in form ; thus the product of a^m and a^n which is a^{m+n} when m and n are whole numbers and therefore general in form though particular in value, will be their product likewise when m and n are general in value as well as in form ; the series for $(a+b)^n$ determined by the principles of arithmetical algebra when n is any whole number, if it be exhibited in a general form, without reference to a final term, may be shown upon the same principle to be the equivalent series for $(a+b)^n$ when n is general both in form and value.''

The principle here brought forward was named by Peacock the ''principle of the permanence of equivalent forms'' ; by means of it the transition is made from arithmetical algebra to symbolical, and at page 59 of '' Symbolical Algebra'' it is thus enunciated : '' Whatever algebraical forms are equivalent, when the symbols are general in form but specific in value, will be equivalent likewise when the symbols are general in value as well as in form.''

One asks naturally, What are the limits set to the generality of the symbol? Peacock's answer is, ' Whatsoever.' In the theory of reasoning the great question is not, ' How do we pass from generals to particulars?' but ' How do we pass from particulars to generals?' The application of general principles is plain enough,—the difficulty is in explaining how we arrive at the truth of the general principles. The logician, seeking for light on this question, is apt to turn to exact science, and especially to algebra, the most perfect branch of exact science. Should he turn to Peacock, he finds that all that is offered him is this ''principle of the permanence of equivalent forms ;'' which, paraphrased, amounts to the following : We find certain theorems to be true when the symbol denotes integer number ; let these theorems be true without restriction, and let us try to find the different interpretations which may be put on the symbol. Is not the following attitude more logical? We find certain theorems to be true, when the symbol denotes number ; how far and no further may the conception of number be generalized yet these theorems remain true without any alteration of form?; and, should the conception of number be still further generalized, what is the

modified form which the theorems then assume? This is the logical process of generalization, whereas Peacock's process is "essentially arbitrary, though restricted with a specific view to its operations and their results admitting of such interpretations, as may make its applications most generally useful." (Report on Recent Progress in Analysis, p. 194.)

The two processes may be illustrated by their application to the binomial theorem, proved to be true for a positive. integer index. According to Peacock's process,

$$(a + b)^n = a^n + na^{n-1} b + \frac{n(n-1)}{1.2} a^{n-2}b^2 +$$

is to be made a theorem in symbolical algebra, whether the series be finite or infinite, and all that remains is to find the different ways in which it may be interpreted. The process of generalization proceeds by steps. For instance, it asks : Will the series retain the same form when n is generalized so as to include any rational fraction? This is one of the questions which Newton proposed to himself, and settled in the affirmative ; and it is recorded that he verified the truth of his conclusion by squaring the series for $(1-x^2)^{\frac{1}{2}}$. Peacock's principle does not distinguish divergent from convergent series; it is nothing but hypothesis, and any result suggested by it must stand the test of independent investigation.

An important advance in the philosophy of the fundamental principles of algebra was made by D. F. Gregory, a younger member of the Cambridge school of mathematicians. Descended from a Scottish family, already famous in the annals of science, he early gave promise of adding additional luster to the name ; this he accomplished in a brief life of 31 years. In 1838 he read a paper before the Royal Society of Edinburgh "On the Real Nature of Symbolical Algebra," in which he says: "The light in which I would consider symbolical algebra is, that it is the science which treats of the combination of operations defined not by their nature, that is, by what they are or what they do, but by the laws of combination to which they are subject. And as many different kinds of operations may be included in a class defined in the manner I have mentioned, whatever can be proved of the class generally, is neces-

sarily true of all the operations included under it. This, it may be remarked, does not arise from any analogy existing in the nature of the operations which may be totally dissimilar, but merely from the fact that they are all subject to the same laws of combination. It is true that these laws have been in many cases suggested (as Mr. Peacock has aptly termed it) by the laws of the known operations of number; but the step which is taken from arithmetical to symbolical algebra is, that leaving out of view the nature of the operations which the symbols we use represent, we suppose the existence of classes of unknown operations subject to the same laws. We are thus able to prove certain relations between the different classes of operations, which, when expressed between the symbols, are called algebraical theorems. And if we can show that any operations in any science are subject to the same laws of combination as these classes, the theorems are true of these as included in the general case; provided always that the resulting combinations are all possible in the particular operation under consideration."

It will be observed that he places algebra on a formal basis; for its symbols are defined, not to represent real operations, but by laws of combination arbitrarily chosen. In a subsequent paper, however, entitled " On a Difficulty in the Theory of Algebra," he practically gave up the formal view, and appears inclined to adopt the realist view instead. He says : " In previous papers on the theory of algebra I have maintained the doctrine that a symbol is defined algebraically when its laws of combination are given; and that a symbol represents a given operation when the laws of combination of the latter are the same as those of the former. This, or a similar theory of the nature of algebra seems to be generally entertained by those who have turned their attention to the subject; but without in any degree leaning on it, we may say that symbols are actually subject to certain laws of combination, though we do not suppose them to be so defined ; and that a symbol representing any operation must be subject to the same laws of combination as the operation it represents." This is a departure from conventional definitions to rules founded upon the universal properties of that which is represented.

In the paper first quoted, Gregory considers five classes of operations. He supposes + and — to be defined by the rules of signs; and he finds in arithmetic a pair of operations which come under it, namely, addition and subtraction; and in geometry another pair, namely, turning through a circumference, and a semicircumference respectively. But it is instructive to note that the difficulty referred to in the title of the later paper is none other than the view that + and — represent the operations of addition and subtraction; and he there shows that addition (including subtraction) is subject to a couple of very different laws, the commutative and the associative, though he does not use the latter term. It may be observed that the rule of signs applies to × and ÷ also; hence if + and — embraced addition and subtraction, so would × and ÷. The truth of the matter is that in ascending from arithmetic to algebra, we replace the coordinate ideas of *addition* and *subtraction* by the more general idea of *sum* and the subordinate functional idea of *opposite*. Similarly the coordinate ideas of multiplication and division are replaced by the more general idea of a *product* and the subordinate functional idea of *reciprocal*. The symbols — and ÷ then denote opposite and reciprocal respectively, while the ideas of sum and product are not expressed by symbols, but are sufficiently indicated by the manner of writing of the several elements. This difficulty appears to have upset his belief in the existence of classes of operations subject to the same laws of combination, yet totally dissimilar in nature, and without any real analogy binding them together.

According to Gregory, the second class of operations are the index operations, subject to the two laws:

$$f_m(a)f_n(a) = f_{m+n}(a) \text{ and } f_m f_n(a) = f_{mn}(a).$$

The third class comprises the ordinary symbol of algebra, and the symbols d and $\varDelta$ of the calculus; they are subject to the distributive law

$$f(a) + f(b) = f(a+b),$$

and to the commutative law

$$f_1 f(a) = f f_1(a).$$

The fourth class comprises the logarithmic operations subject to the law

$$f(a) + f(b) = f(ab).$$

The fifth class are the sine and cosine functions, subject to the laws expressed by the fundamental theorem of plane trigonometry, namely, the connection between the sine and cosine of the sum of two angles and the sines and cosines of the component angles.

Following as far as may be the chronological order, we come next to Augustus De Morgan, distinguished for his contributions alike to logic and to mathematics. In his "Formal Logic" he takes a formal view of the nature of reasoning in general, and in his "Trigonometry and Double Algebra" he lays down an excessively formal foundation for algebra. Indeed it may be said that he carries formalism to its logical issue; and thereby he renders a service, for its inadequacy then becomes the better evident. In the chapter of the book mentioned, which is headed, "On Symbolic Algebra," he thus expresses the view he had arrived at: " In abandoning the meanings of symbols, we also abandon those of the words which describe them. Thus addition is to be, for the present, a sound void of sense. It is a mode of combination represented by $+$; when $+$ receives its meaning, so also will the word addition. It is most important that the student should bear in mind that, *with one exception*, no word nor sign of arithmetic or algebra has one atom of meaning throughout this chapter, the object of which is symbols, and their laws of combination, giving a symbolic algebra which may hereafter become the grammar of a hundred distinct significant algebras. If any one were to assert that $+$ and $-$ might mean reward and punishment and A, B, C, etc., might stand for virtues and vices, the reader might believe him, or contradict him, as he pleases, but not out of this chapter. The one exception above noted, which has some share of meaning, is the sign $=$ placed between two symbols, as in $A = B$. It indicates that the two symbols have the same resulting meaning, by whatever steps attained. That A and B, if quantities, are the same amount of quantity ; that if operations, they are of the same effect, etc." Let us apply to the theory quoted the logical maxim that the exception proves the rule, *prove* being used in the old sense of test. Well then, I say, because one symbol at least is found to be refractory to the theory, it follows that the theory is fallacious.

De Morgan proceeds to give an inventory of the fundamental symbols and laws of algebra, that for the symbols being o, 1, +, —, ×, ÷, ( )$^{( )}$ and letters. With respect to it the following questions may be asked : Why should ( )$^{( )}$ be included, while the inverse idea, denoted by *log* is left out? What of the functional symbols *sin* and *cos*? Can they be derived from the above? As — denotes opposite and ÷ reciprocal, what are the signs for sum and product? Can they be derived from the above?

His inventory of the fundamental laws is expressed under 14 heads, but some of them are merely definitions. The laws proper may be reduced to the following, which he admits are not all independent of one another :

I. Law of signs : $+ + = +, + - $ or $ - + = -, - - = +$.
  $\times \times = \times$, $\times \div$ or $\div \times = \div$, $\div \div = \times$.

II. Commutative law : $a + b = b + a$, $ab = ba$.

III. Distributive law : $a(b + c) = ab + ac$.

IV. Index laws : $a^b \times a^c = a^{b+c}$, $(a)^c = a^{bc}$, $(a^b)_c = a^c b^c$.

V. $a - a = 0$, $a \div a = 1$.

These last may be called the rules of reduction. What Gregory gave was a classification of the more important operations occurring in algebra; De Morgan professes to give a complete inventory of the laws which the symbols of algebra must obey, for he says ''Any system of symbols which obeys these rules and no others, except they be formed by combination of these rules, and which uses the preceding symbols and no others, except they be new symbols invented in abbreviation of combinations of these symbols, is symbolic algebra.''

Compare this inventory with Gregory's classification. De Morgan brings × and ÷ under the same rule as + and — ; he applies the commutative law to a sum as well as to a product; he introduces the third index law, which makes the index distributive over the factors of the base ; he leaves out the logarithmic and trigonometrical principles and introduces what may be called the rules of reduction. From his point of view, none of them are rules; they are laws, that is, arbitrarily chosen relations to which the algebraic symbols must be subject. He does not mention the law pointed out by Gregory, afterwards called the law of association. It is an unfortunate

thing for the formalist that a^b is not equal to b^a , for then his commutative law would have full scope ; as it is, the index operations prove exceedingly refractory, so that in some of the beautifully formal systems they are left out of account altogether.   Here already we have sufficient indication that to give an inventory of the laws which the symbols of algebra *must* obey, is as ambiguous a task as to give an inventory of the *a priori* furniture of the mind.

Like De Morgan, George Boole was a mathematician who investigated and wrote in the field of logic.   The character of the work done by the two men is very different ; De Morgan's work bristles with new symbols, Boole uses only the familiar symbols of analysis ; the former polished many small stones, the latter raised an edifice of grand proportions.   The work done by Boole in applying mathematical analysis to logic necessarily led him to consider the general question of how reasoning is accomplished by means of symbols.   The view which he adopted on this point is stated at page 68 of the " Laws of Thought."

" The conditions of valid reasoning by the aid of symbols, are : *First*, that a fixed interpretation be assigned to the symbols employed in the expression of the data ; and that the laws of the combination of those symbols be correctly determined from that interpretation ; *Second*, that the formal processes of solution or demonstration be conducted throughout in obedience to all the laws determined as above, without regard to the question of the interpretability of the particular results obtained ; *Third*, that the final result be interpretable in form, and that it be actually interpreted in accordance with that system of interpretation which has been employed in the expression of the data."

As regards these conditions it may be observed that they incline towards the realist view of analysis.   True he speaks of interpretation instead of meaning, but it is a fixed interpretation ; and the rules for the processes of solution are not to be chosen arbitrarily, but are to be found out from the particular system of interpretation of the symbols.   The thoroughgoing realist view is, that a symbol stands for some definite notion in the subject analyzed, and that the rules of the analysis are

founded upon universal properties of the subject analyzed. The realist view of mathematical science has commended itself to me ever since I made an exact analysis of relationship, and devised a calculus which provides a notation for any relationship; can express in the form of an equation the relationship existing between any two persons, and provides rules by means of which a single equation may be transformed, or a number of equations combined so as to yield any relationship involved in their being true simultaneously. The notation is made to fit the subject, and the rules for manipulation are derived from universal physiological laws, and the more arbitrary laws of marriage. The basis is real; yet the analysis has all the characteristics of a calculus, and throws light by comparison on several points in ordinary algebra. Its fundamental symbol expresses a relation; and what is the ultimate meaning of the algebraical symbol or of the symbol of the calculus but an operation or relation?

It is Boole's second condition which principally calls for study and examination; respecting it he observes as follows: "The principle in question may be considered as resting upon a general law of the mind, the knowledge of which is not given to us *a priori*, *i. e.*, antecedently to experience, but is derived, like the knowledge of the other laws of the mind, from the clear manifestation of the general principle in the particular instance. A single example of reasoning, in which symbols are employed in obedience to laws founded upon their interpretation, but without any sustained reference to that interpretation, the chain of demonstration conducting us through intermediate steps which are not interpretable to a final result which is interpretable, seems not only to establish the validity of the particular application, but to make known to us the general law manifested therein. No accumulation of instances can properly add weight to such evidence. It may furnish us with clearer conceptions of that common element of truth upon which the application of the principle depends, and so prepare the way for its reception. It may, where the immediate force of the evidence is not felt, serve as verification, *a posteriori*, of the practical validity of the principle in question. But this does not affect the position affirmed, *viz.*, that the general

principle must be seen in the particular instance—seen to be general in application as well as true in the special example. The employment of the uninterpretable symbol $\sqrt{-1}$ in the intermediate processes of trigonometry furnishes an illustration of what has been said. I apprehend that there is no mode of explaining that application which does not covertly assume the very principle in question. But that principle, though not, as I conceive, warranted by formal reasoning based upon other grounds, seems to deserve a place among those axiomatic truths which constitute in some sense the foundation of general knowledge, and which may properly be regarded as expressions of the mind's own laws and constitution." (p. 68.)

We are all familiar with the fact that algebraic reasoning may be conducted through intermediate equations without requiring a sustained reference to the meaning of these equations; but it is paradoxical to say that these equations can, in any case, have no meaning, no sense, no interpretation. It may not be necessary to consider their meaning, it may even be difficult to find their meaning, but that they have a meaning is a dictate of common sense. It is entirely paradoxical to say that, as a general process, we can start from equations having a meaning and arrive at equations having a meaning by passing through equations which have no meaning. The particular instance in which Boole sees the truth of the paradoxical principle is the successful employment of the uninterpretable symbol $\sqrt{-1}$ in intermediate processes of trigonometry. So soon then as the $\sqrt{-1}$ occurring in these processes is interpreted, or rather so soon as its meaning is demonstrated, the evidence for the principle fails. As a matter of fact the doctrine of algebraists about $\sqrt{-1}$ has long been a dark corner in exact science; and as a consequence it has been made the foundation for all sorts of crank theories. Recently I noticed that an ingenious individual had applied the $\sqrt{-1}$ and its successive powers to construct a mathematical theory of sensation. Before the introduction by Descartes of the geometrical idea of the opposite, the use of $-$ in algebra might have been made the foundation for a similar transcendental theory of reasoning. Algebra, as the analysis of quantity in space has a clear meaning for $\sqrt{-1}$ as the operation of turning through a right

angle round a definite or an indefinite axis; in the former case it is vector in nature, because the axis must be specified; in the latter it is scalar in nature, because the axis may be any suitable one. It follows that — denotes turning through two right angles, and this includes 'opposite' as a particular case. Thus an instance is still wanting on which to build the transcendental theory of reasoning enunciated by Boole.

The object of Boole's work, "The Laws of Thought," is to investigate the fundamental laws of thought, to give expression to them in the symbolical language of a calculus, and upon that foundation to establish the science of logic. In the concluding chapter he considers the light which the inquiry throws on the nature and constitution of the human mind. Now, as a matter of fact, the subject analyzed is quality, and its connection with the nature and constitution of the human mind is nowise more intimate than is the connection of algebra the science of quantity.

It is interesting to compare Boole's inventory of the symbols and laws for a calculus of reasoning (analysis of quality) with the inventory made by De Morgan for the symbols and laws of algebra (the analysis of quantity). The symbols are the same, excepting that ( )$^{( )}$ is omitted. The law of signs, for + and — is the same, but none is given for × and ÷ on account of the ambiguity of the reciprocal; the commutative law applies to both sum and product; the distributive law applies to the product of sums; there are no index laws, excepting the peculiar one $a^2 = a$; the law of reduction $a - a = o$ remains, but the complementary law $\frac{a}{a} = 1$ is not true in general.

How is the truth or suitability of these laws established? He says that it would be mere hypothesis to borrow the notation of the analysis of quantity, and to assume that in its new application the laws by which its use is governed would remain unchanged; to establish them he investigates the operations of the mind in reasoning as expressed by language, and applies Kant's theory of seeing the general truth in a particular instance. As regards the commutative law it may be remarked that Boole overlooks the fact that two notions may in their

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definition be coordinate with one another, or subordinate the one to the other, just as in the theory of probability there is a difference between two events which are independent of one another, and two events which are dependent the one on the other; and in the latter case it is not true that the order of the notions is indifferent. This is not the place to enter into a discussion of these so-called laws of thought; I wish merely to point out that Boole's view is essentially that of the realist; the fundamental rules of an analysis are not to be assumed arbitrarily, but must be found out by investigation of the subject analyzed.

Cotemporaneously with Boole, and living on the same Emerald Isle, another mathematician spent many days reflecting on the fundamental principles of algebra—Sir W. R. Hamilton. His investigation started from the reading of some passages in Kant's "Critique of the Pure Reason," which appeared to justify the expectation that it should be possible to construct *a priori* a science of time as well as a science of space. The principal passage is as follows: "Time and space are two sources of knowledge from which various *a priori* synthetical cognitions can be derived. Of this pure mathematics gives a splendid example in the case of our cognitions of space and its various relations. As they are both pure forms of sensuous intuition, they render synthetical propositions *a priori* possible." Thus, according to Kant, space and time are forms of the intellect; and Hamilton reasoned that, as geometry is the science of the former, so algebra must be the science of the latter. He amplifies that view as follows: " It early appeared to me that these ends might be attained by our consenting to regard algebra as being no mere art, nor language, nor primarily a science of quantity; but rather as the science of order in progression. It was, however, a part of this conception, that the progression here spoken of was understood to be continuous and unidimensional; extending indefinitely forward and backward, but not in any lateral direction. And although the successive states of such a progression might, no doubt, be represented by points upon a line, yet I thought that their simple successiveness was better conceived by comparing them with moments of time, divested, however, of all reference to cause and effect;

so that the 'time' here considered might be said to be abstract, ideal, or pure, like that 'space' which is the object of geometry.   In this manner I was led to regard algebra as the science of pure time, and an essay, containing my views respecting it as such, was published in 1835." (Preface to "Lectures on Quaternions," p. 2.)   If algebra is based on any unidimensional subject, a difficulty arises in explaining the roots of a quadratic equation when they are imaginary.   To get over the difficulty Hamilton invented a theory of algebraic couplets, but the success of the invention is doubtful.   In his presidential address before the British Association, the late Professor Cayley said that he could not appreciate the manner in which Hamilton connected algebra with the notion of time, and still less could he appreciate the manner in which he connected his algebraical couplet with the notion of time.   Whether Hamilton has effected the explanation or not, it appears to be logically possible ; for a complex quantity can be represented by two segments of one and the same straight line.

But, be that as it may, Hamilton was led from algebraic couplets to algebraic triplets, and to the problem of adapting triplets to the representation of lines in space.   His guiding idea was to extend to space the mode of multiplication of lines in a plane already discovered by Argand, Warren, and others ; and it was here that he stepped from the time basis to the space basis, that is, passed from a unidimensional to a tridimensional subject, the latter including the former as a special case.   To his surprise he found that the multiplication of two lines in space, either one being expressed in terms of three elements, led to a product composed not of three, but of four elements ; and this result he deemed so novel and characteristic that he selected it to give a name to the new method—"Quaternions." As finally developed the method rests on a geometrical basis ; nevertheless it is the logical generalization of ordinary algebra, for the distinctive theorems of algebra, such as the exponential, binomial and multinomial theorems, have their generalized counterparts in quaternions.   Since the time of Gauss, mathematicians have considered double or plane algebra to be the logical generalization of ordinary algebra ; now quaternions bears to plane algebra the same logical relation which plane

algebra bears to ordinary algebra.   It is all algebra in the sense
of being the analysis of quantity and the relations of quanti-
ties.   Any one who admits De Moivre's theorem into alge-
bra, is logically bound to admit quaternions as the highest
form of algebra.   It is a common belief that quaternions has
only a remote connection with algebra ; that it is only one of
several systems of non-commutative algebra, and that the
mathematician can get on very well without it.   But if the
above is the true logical relation, then it must be the duty of
every analyst to master its principles.   It may be remarked
here that the logical relation of quaternions to plane algebra is
obscured by the prevalent but erroneous idea that the complex
quantities of the form $x + iy$ represent vectors.   They really
represent, in their planar meaning, coaxial quaternions ; that
is, x is a scalar and the axis of y is the common perpendicular
to the plane.   Let, as usual, $w + ix + jy + kz$ denote a qua-
ternion ; the complex quantity is identical not with $w + ix$ or
$ix + jy$, but with $w + kz$.   The fallacy in question almost
baffled Hamilton in his attempts at generalization, as may be
seen from the account which he gives of the discovery in the
Philosophical Magazine for 1844.

We shall obtain additional insight into the nature of the
fundamental laws of algebra, by considering the part which
they played in the discovery of the quaternion generalization.
In the endeavor to adapt the general conception of a triplet
to the multiplication of lines in space, Hamilton started out
with the principles of commutation, distribution, and reduction;
but in order that the theorem about the moduli might remain
true, he soon felt obliged, not indeed to abandon the principle
of commutation entirely, but to modify it so as to preserve the
order of the factors while leaving the order of combination of
the factors commutable.   This principle, which had previously
been pointed out by Gregory as an independent principle, he
called the law of association.   As the principle of commuta-
tion was still assumed to apply to the terms of a sum, it fol-
lowed that the principle of association also applied to them.
Here then we have an important difference in the inventory
of the laws of algebra.   According to De Morgan algebra fol-
lows all the laws which he enumerated, and them only ; but

Hamilton showed that the legitimate extension of algebra to space requires the commutative law to be modified in the case of a product. And still further light is obtained on the nature of these laws, by considering the way by which Hamilton satisfied himself of the truth of the principle of association. He sought for and obtained a geometrical proof, independent of the principle of distribution, and depending on theorems taken from spherical trigonometry or spherical conics. Thus a notable generalization of algebra was made, not by arbitrary choice of fundamental rules, nor by arbitrary extension of the rules for integer number, but by finding out the universal properties of the subject analyzed.

We have already found that the index operations form a valuable test of the soundness of any theory of algebra. If the method of quaternions is the true extension of algebra to space, we expect it to throw new light on these operations. As a matter of fact most of the works on quaternions ignore the subject, or present instead the treatment for the plane. In Hamilton's "Elements of Quaternions" there is a chapter headed "On Powers and Logarithms of Diplanar Quaternions," but what it contains is practically limited to the plane. Why? Because the author believed, and there states, that the fundamental exponential law is not true for diplanar quaternions, that is, for space

$$e^p \times e^q \text{ not } = e^{p+q}.$$

The source of the error lies in regarding the sum of indices as commutative, for that amounts to holding that $e^p \times e^q = e^q \times e^p$, which is contrary to the principles of quaternions. Were $p + q$ a sum without any real order of the terms, then we might have an order of factors, that is, we might have

$$(p + q)(p + q) = p^2 + pq + qp + q^2 = p^2 + q^2 + 2Spq.$$

But when the sum has a real order of p prior to q, then we cannot at the same time hold that one factor $p + q$ can be prior to another factor $p + q$; for in the expansion we would have the contradiction of p being prior to q and q at the same time prior to p. Hence, when p is prior to q the second power is not formed in accordance with the distributive principle; it is $p^2 + 2pq + q^2$. When this is admitted, the exponential principle stands, but the commutative principle for a sum of

such indices goes, as does also the distributive manner of forming the powers of such a sum.

As regards the third index law, it is evident from the noncommutability of the factors in general, that in space it ceases to be true. The rule of reduction for a sum of terms requires to be modified when the terms have a real order; for $p + q - q = p$, but $q + p - q$ is not equal to p. The term and its opposite must follow one another immediately, in order that the reduction may be legitimate. Similarly, in the case of a product, the factor and its reciprocal must follow one another immediately in order that the reduction may be legitimate. From these principles the generalization for space of all the fundamental theorems of algebra follows without difficulty, and the theory of logarithms and exponents becomes the most fruitful part of quaternion analysis.

We may now consider briefly how the advance made by Hamilton struck a cotemporary mathematician—Prof. Kelland, of the University of Edinburgh. It was his custom to teach the elements of quaternions to the students of his senior class, and I remember how all went well till he came to multiplication, where the part played by a vector as a multiplier was likened in some mysterious manner to the action of a corkscrew. In the introductory chapter of the "Introduction to Quaternions," he remarks as follows on the process by which algebra is generalized : "It is only by standing loose for a time to logical accuracy that extensions in the abstract sciences —extensions at any rate which stretch from one science to another—are effected." And further on : "We trust, then, it begins to be seen that sciences are extended by the removal of barriers, of limitations, of conditions, on which sometimes their very existence appears to depend. Fractional arithmetic was an impossibility so long as multiplication was regarded as abbreviated addition ; the moment an extended idea was entertained, ever so illogically, that moment fractional arithmetic started into existence. Algebra, except as mere symbolized arithmetic, was an impossibility so long as the thought of subtraction was chained to the requirement of something adequate to subtract from. The moment Diophantus gave it a separate existence—boldly and logically as it happened—by

exhibiting the law of *minus* in the forefront as the primary definition of his science, that moment algebra in its highest form became a possibility, and indeed the foundation-stone was no sooner laid than a goodly building arose on it."

It seems to me that no greater paradox could be enunciated than to say that higher principles in exact science are reached by standing loose for a time to logical accuracy. How long a time does that which is illogical take to become logical? The true process is generalization, not illogical extension. No doubt the generalized principle may at first be merely an hypothesis, and in that form it may be applied so that it may be verified by its results; but this is not standing loose to logical accuracy.

The same author gives the following account of how Hamilton *extended* algebra to space: "He had done a considerable amount of good work, obstructed as he was, when about the year 1843, he perceived clearly the obstruction to his progress in the shape of an old law, which prior to that time, had appeared like a law of common sense. The law in question is known as the commutative law of multiplication. Presented in its simplest form it is nothing more than this: 'five times three is the same as three times five'; more generally, it appears under the form of $ab = ba$ whatever a and b may represent. When it came distinctly into the mind of Hamilton that this law is not a necessity with the extended signification of multiplication, he saw his way clear, and gave up the law. The barrier being removed, he entered on the new science as a warrior enters a besieged city through a practicable breach." This account is of course inadequate, for Grassmann jumped over the same barrier in the shape of an "old law," yet he was unable to deal with angles in space. There is no occasion to speak disrespectfully of the law of commutation; it has its own place; Hamilton did not cast it aside as an obstruction; he modified it for a product of factors having a real order, and the modified form amounts to the law of association.

We shall now go back to another independent source of the development of the principles of algebra—Hermann Grassmann. Like his cotemporary, Hamilton, he was remarkable alike for attainments in mathematics and philosophy, and, besides, he

made important contributions to philology. No doubt specialists are necessary, but the investigation of the fundamental principles of a science requires one who is more than a specialist, one who has not only studied a portion minutely, but has also taken a comprehensive glance over the whole. From the preface to the *Ausdehnungslehre* of 1844 we get an insight into the origin and development of his course of investigation, and we find that it was in a manner the reverse of that of Hamilton. The former started from a variety of geometrical facts, and developed a method which is independent of space, and has perhaps suffered from its *philosophische Allgemeinheit;* the latter started from general philosophical ideas and developed an algebra which is uniquely adapted to space of three dimensions. But as their subjects were largely the same, their results, so far as they involve truth, must also be capable of unification to a large extent.

In the preface quoted Grassmann informs us that he started from the treatment of negatives in geometry ; he observed that the straight lines AB and BA were opposite, and that AB + BC = AC whether the point C is beyond B, or between A and B. This led him to the principle of geometrical addition, namely, that AB + BC = AC, whether A, B, C are in one straight line or not. It may be remarked here that this principle is all right, so long as the components have no real order, such as forces applied at a point, or the coordinates of a point ; but that it does not apply where the components have a real order, as for example, the sides of a polygon. In successive addition the straight line from the origin to the end of the polygon is the scalar result, but the area enclosed is another result, which depends on the form of the path.

Then turning to the product in geometry, he adopted the view that the parallelogram is the product of its two sides, whether these are at right angles or not. He next found that the geometrical ideas of a sum and a product which he had adopted, satisfied the principle of distribution, but not the principle of commutation so far as the factors of a product were concerned. In the case of the products commutation could be made, provided the sign of the product were changed also, that is, they were subject to negative commutation. An

other set of basal facts were taken from the doctrine of the
center of gravity.   He observed that the center of gravity may
be considered as the sum of several points, the line joining
two points as the product of the points, the triangle as the
product of its three points, and the pyramid as the product of
its four points, and from these facts he developed a method
similar to the "Barycentric Calculus," of Möbius.

He also considered the geometrical meaning of the exponen-
tial function.   He observed that if a denote a finite straight
line and a an angle in a plane through the line, then ae^a de-
notes the line a turned through the angle a.   The treatment
of angles in one plane is easy, but on attempting to treat of
angles in space, he encountered difficulties which he was un-
able to surmount.   This fact has been cited as indicating the
superiority of Hamilton's method ; while that is true, it must
not be forgotten that Hamilton failed to generalize the expo-
nential theorem.

What is the view which Grassmann takes of the funda-
mental principles of algebra?   An answer to this question is
found in the introduction to the *Ausdehnungslehre* of 1844.
He divides the sciences into the real and the formal ; the for-
mer treat of reality, and their truth consists in the agreement
of thought with reality ; the latter treat of thought only, and
their truth consists in the agreement of the processes of thought
with one another.   Pure mathematics is the doctrine of forms.
As a consequence he is obliged to place geometry under ap-
plied mathematics ; for it has a real subject, and should any-
one think otherwise he must deduce from pure thought the
tridimensional character of space.   Were space a form of
thought, so would be time and motion, and kinematics would
also be a part of pure mathematics.   So he relegates geometry
to the real sciences ; and he has a difficulty in retaining arith-
metic even ; for is it not based on axioms, whereas a formal
science is based on conventions?

From the notion of the combination of terms he deduces
that the placing of the brackets and the order of the terms may
or may not be indifferent.   There is a synthetic combination
and an analytic combination ; when the latter is unambiguous
(that is, $a - a = o$), then the placing of the brackets and the

order of the terms is indifferent ; synthetic combination is then called addition, and the analytic subtraction. Thus in Grassmann's view, the commutative and associative laws are involved in the ideas of addition and subtraction. It may be observed that the old difficulty with subtraction is due to the fact that it is not thoroughly commutative, and that it is only to the generalized idea of composition that the commutative law applies. Besides, to define addition so as to exclude terms having a real order, is an arbitrary restriction of algebra.

According to Grassmann's view multiplication is a combination of a higher order ; that is, he assumes as the definition of multiplication the distributive principle in the two-fold form,

$$(a + b)c = ac + bc \text{ and } c(a + b) = ca + cb.$$

It may be observed, however, that the true expression for the distributive principle is

$$(a + b)(c + d) = ac + ad + bc + bd$$

which assumes that if there is any real order of the terms, there can be only one real order $a\ b\ c\ d$.

As regards the laws of indices, he says that involution is a combination of the third order, and that for the sake of shortness he will omit all consideration of it. Besides, its formal definition would be of no use, for in the nature of things it can be applied only in the special sciences through real definitions. This failure to treat of the index laws tells against his whole theory of the nature of algebra. In fact, these laws are the touchstone whereby the soundness of any theory of the foundations of algebra may be tested.

In 1867 Hermann Hankel published his "Theory of Complex Numbers." The full title of the work is " *Theorie der complexen Zahlensysteme insbesondere der gemeinen imaginären Zahlen und der Hamilton'schen Quaternionen nebst ihrer geometrischen Darstellung.*" He had studied the writings of both Hamilton and Grassmann, and the aim of the book is to give a complete theory of the several systems, uniting them all under the notion of complex number. From the title we gather that he considered the algebraic imaginaries and the Hamiltonian quaternions as two distinct systems, formal in their nature, but having a representation in space. He begins with positive integer numbers, and finds from a consideration of the notion

that the addition of such numbers satisfies the two laws of association and commutation, which he treats as independent of one another. But as regards the notion of the multiplication of such numbers, he says that the truth of the commutative law or of the associative law is not self-evident; that the former law can be proved by a geometric construction in a plane, and the latter by a geometric construction in space. As regards the distributive law he says merely that it is a universal property of multiplication. As regards the base and index relation he says that neither the commutative law nor the associative law applies; he enunciates the same three index laws as De Morgan, but does not say whether they are self-evident or require a proof by geometric construction. Here then in a professedly scientific work, some of these fundamental laws are treated as self-evident, others as requiring geometric proof, and others yet are merely enunciated. If in the case of multiplication the commutative law requires proof, so does it also in the case of addition; for it is just as self-evident that $2 \times 3 = 3 \times 2$ as that $2 + 3 = 3 + 2$.

The manner in which Hankel passes from arithmetic and arithmetical algebra to general algebra is as follows: Algebra, being formal mathematics, can be founded on any system of independent rules; but in order that its results may be interpretable and that it may be capable of application, it is found convenient to choose the system of fundamental rules satisfied by common arithmetic; in other words, the laws of integer arithmetic are made the laws of algebra. This he calls the "principle of the permanence of the formal laws," and enunciates as follows (p. 11): "If two expressions stated in terms of the general symbols of arithmetical algebra (*arithmetica universalis*) are equal to one another, they shall remain equal to one another, when the symbols cease to denote simple magnitudes and the operations receive any other meaning." Peacock speaks of the permanence of equivalent forms; Hankel, of the permanence of the formal laws. Peacock says, "let any general equivalence in arithmetical algebra be true also in universal algebra;" Hankel says, "let the fundamental laws of the former be made the fundamental laws of the latter." Hankel gives a more scientific form to what was meant by Peacock.

However, Hankel labors under a logical difficulty from which Peacock was exempt ; for he does not take the laws of arithmetical algebra without exception ; he rejects the commutative law for a product, in order that quaternions may be included among his complex numbers. But, it may be asked, why not reject the commutative law for addition also ; so far as arithmetical algebra is concerned, they stand on the same basis. If, as has been shown, the sum of quaternion indices is not commutative, we are logically bound, on his principles, to reject the commutative rule for addition also. We are reduced to the alternative ; the choice of the fundamental rules is arbitrary, or else they must be founded on the properties of the subject analyzed. The permanence of the formal laws is nothing but hypothesis, and in the case of any generalization must be tested by real investigation.

One of the clearest thinkers on mathematical subjects in recent times was Professor Clifford, who like several of the mathematical philosophers we have spoken of, was cut down in the midst of his scientific activity. In his posthumous work entitled "The Common Sense of the Exact Sciences," there are chapters on number and quantity, in which he explains his view of the fundamental principles of algebra. He starts out from the principle, which he attributes to Cayley and Sylvester, that the number of any set of things is the same in whatever order we count them ; and deduces from it by means of diagrams the commutative and associative rules for positive integer number. He says that they amount to the following : " If we can interchange any two consecutive things without altering the result, then we may make any change whatever in the order without altering the result." It may be remarked that this shows that the commutative and associative properties are not independent, but that the former involves the latter. He next shows by a diagram that the distributive rule is true for the two forms $a(b + c) = ab + ac$ and $(b + c)a = ba + ca$, but he does not consider the complete form of the rule $(a + b)(c + d) = ac + ad + bc + bd$.

As regards the impossible subtraction and division he says (p. 33) : "Every operation in mathematics that we can invent amounts to asking a question, and this question may or may

not have an answer according to circumstances. If we write down the symbols for the answer to the question in any of those cases where there is no answer, and then speak of them as if they meant something, we shall talk nonsense. But this nonsense is not to be thrown away as useless rubbish. We have learned by very long and varied experience that nothing is more valuable than the nonsense which we get in this way; only it is to be recognized as nonsense, and by means of that recognition made into sense. We turn the nonsense into sense by giving a new meaning to the words or symbols which shall enable the question to have an answer that previously had no answer.''

This is the true phenomenon in algebra; it is more logical than its framer. How can it be possible, unless the algebraist founds his analysis upon real relations? It is the logic of real relations which may outrun the imperfect definitions and principles of the analyst, and make it necessary for him to return to revise them.

To get over the impossible subtraction he introduces instead of the discrete unit supposed by number, the idea of a step, making plus mean "forwards" and minus "backwards." The summing of steps is independent of the order in which they are taken, and a minus step is just as independent as a plus step. When these symbols occur in multipliers, he gives them, not the meaning of "forwards" and "backwards," but that of "keep" and "reverse." He gives them these meanings in addition to their former meanings, and leaves it to the context to show which is the right meaning in any particular case. It may be remarked that it is doubtful whether in any case two distinct meanings can be given to a symbol at one and the same time, without producing confusion. It seems to me, as already stated, that the most general meanings of + and — are the angular ideas of an even and an odd number of semi-circumferences, but this reduces in certain cases to the linear ideas of direct and opposite.

From the idea of step he passes to the idea of operation, on the theory that a product may be composed either of a step and an operation, or of two operations. As a matter of fact an operation is merely a relationship which may subsist be-

tween two quantities; and we may have two distinct products, one expressing a related quantity, the other a compound relationship. The analysis of operations is a special part of the more general analysis of relationships. According to Clifford's view, because a sum of operations of the kind considered is independent of the order of the operations, it follows that

$$a + b = b + a \qquad\qquad ab = ba$$
$$a(b + c) = ab + ac \qquad (a + b)c = ac + bc.$$

As regards the advance from numbers to quantity he says ("Philosophy of the Pure Sciences," p. 240) : "For reasons too long to give here, I do not believe that the provisional use of unmeaning arithmetical symbols can ever lead to the science of quantity; and I feel sure that the attempt to found it on such abstractions obscures its true physical nature. The science of number is founded on the hypothesis of the distinctness of things; the science of quantity is founded on the totally different hypothesis of continuity. Nevertheless, the relations between the two sciences are very close and extensive. The scale of numbers is used, as we shall see, in forming the mental apparatus of the scale of quantities, and the fundamental conception of equality of ratios is so defined that it can be reasoned about in terms of arithmetic. The operations of addition and subtraction of quantities are closely analogous to the operations of the same name performed on numbers and follow the same laws. The composition of ratios includes numerical multiplication as a particular case, and combines in the same way with addition and subtraction. So close and far-reaching is this analogy, that the processes and results of the two sciences are expressed in the same language, verbal and symbolical, while no confusion is produced by this ambiguity of meaning, except in the minds of those who try to make familiarity with language do duty for knowledge of things."

What is the analogy here spoken of ? It cannot be a mere rhetorical analogy; it is a true logical analogy. But what is a logical analogy, except that the two subjects have something in common, which is the basis of the common properties. The logical relation of number to quantity is that of subordination; we cannot pass deductively from the former to the latter, but we can pass deductively from the latter to the former. It is

easy to pass downwards from quantity to number; the difficulty is in passing upwards from number to quantity.

The most elaborate treatise on algebra written in the English language within recent times, is Chrystal's "Text-book of Algebra," published in two volumes. The task which the author sets before himself is the same as that which Peacock undertook, namely, to place the teaching of the elements of algebra on a scientific basis, and abreast of what may be called the technical knowledge of the day. In the first volume he starts out with the idea of building up the science on the three laws of association, commutation and distribution, the two former being applicable to addition and subtraction, multiplication and division, and the third to multiplication. The view which he takes of these laws is expressed by the phrase "canons of the science," as is evidenced by the following passage: "As we have now completed the establishment of the fundamental laws of ordinary algebra, it may be well to insist once more upon the exact position which they hold in the science. To speak, as is sometimes done, of the proof of these laws in all their generality, is an abuse of terms. They are simply laid down as the canons of the science. The best evidence that this is their real position is the fact that algebras are in use whose fundamental laws differ from those of algebra. In the algebra of quaternions, for example, the law of commutation for multiplication and division does not hold generally."

If it is an abuse of terms to speak of the proof of these laws, why does Hamilton devote page upon page to the proof of the associative law for a product of quaternions? He is not content with laying it down as a canon, he investigates whether it corresponds to nature. No doubt the function of the expositor is different from that of the investigator; the latter must establish principles in the best way he can, the former may proceed deductively from these principles as the axioms of the science. But the idea of "canon" involves something arbitrary and formal which is not involved in the idea of an "axiom."

But if we turn to the second volume we find evidence against the canonical nature of these laws, for the author admits that they must be modified within the bounds of algebra itself.

The law of association canot be applied to the terms of an infinite series, unless it is convergent ; the law of commutation cannot be applied to the terms of an infinite series, unless it is absolutely convergent ; and the law of distribution requires modification when applied to the product of two infinite series, If, in any case, the so-called canons are modified, there must be some higher authority to which appeal is made. The only conclusion left is that the rules in question are not canons at all, excepting in so far as they represent properties of the subject analyzed.

I may here refer to the prevalent doctrine that the number-system of arithmetic closes with the complex number, and that the operations of algebra give no indication of any higher imaginary form. For instance, in an article on '' Monism in Arithmetic,'' Prof. Schubert says : ''In the numerical combination $a + ib$, which we also call number, we have found the most general numerical form to which the laws of arithmetic can lead, even though we wished to extend the limits of arithmetic still further. . . . With respect to quaternions which many might be disposed to regard as new numbers, it will be evident that though quaternions are valuable means of investigation in geometry and mechanics they are not numbers of arithmetic, because the rules of arithmetic are not unconditionally applicable to them.'' When the plane of the complex quantity is that of the axes of x and y, it is true that no higher form appears, because in multiplication we get only k and k^2 which is -1. But when Hamilton took for the common plane a general plane passing through the axis of x, he immediately encountered a higher form jk, and the problem resolved itself into finding the meaning of that new imaginary combination. He had a great difficulty in emerging out of '' Flatland,'' but he succeeded in doing it. The reason given for excluding the quaternion cannot apply, for it would exclude infinite series, as the rules of arithmetic are not unconditionally applicable to them.

Last year there appeared the first volume of a '' Treatise on Universal Algebra,'' by Mr. Whitehead, of Trinity College, Cambridge. By universal algebra the author means the various systems of symbolic reasoning allied to ordinary algebra,

the chief examples being Hamilton's Quaternions, Grassmann's Calculus of Extension, and Boole's Symbolic Logic. The author does not include ordinary algebra in his treatment, and the main idea of the work is not unification of the methods, nor generalization of algebra so as to include them, but a detailed study of each structure to be followed by a comparative anatomy. In this idea of comparative anatomy there is involved the assumption that these methods are essentially distinct and independent. But that they overlap to a large extent is very evident.

The author preaches the view of the extreme formalist; nevertheless, at various places he makes admissions which are very damaging to it. As regards the fundamental rules he says : "The justification of the rules of inference in any branch of mathematics is not properly part of mathematics, it is the business of experience or philosophy. The business of mathematics is simply to follow the rules. In this sense all mathematical reasoning is necessary, namely, it has followed the rule." Must the mathematician wait for the experimenter or the philosopher to justify the rules of algebra? Was it no part of Hamilton's business to test whether the associative law is true of a product of spherical quaternions? To advance the principles of analysis is surely the special work of the mathematician; to follow the rules discovered is work of a lower order.

Mr. Whitehead thus describes a calculus : "In order that reasoning may be conducted by means of substitutive signs, it is necessary that rules be given for the manipulation of the signs. The rules should be such that the final state of the signs after a series of operations according to rule denotes, when the signs are interpreted in terms of the things for which they are substituted, a proposition true for the things represented by the signs. The art of manipulation of substitutive signs according to fixed rules, and of the deduction therefrom of true propositions is a calculus." By substitutive sign is meant one such that in thought it takes the place of that for which it is substituted. He quotes with approval a saying of Stout's that a word is an instrument for thinking about the meaning which it expresses; whereas a substitutive sign is a

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means of not thinking about the meaning which it symbolizes; and he adds that the use of substitutive signs in reasoning is to economize thought.

It seems to me that a sign economizes thought in precisely the same way that a word economizes thought, but to a greater degree. A word is introduced to dispense with a long phrase or description, and in using the word one no more thinks of its meaning than in using an algebraic symbol, does one think of the particular meaning it is made to stand for, for the time being. There seems to be a lurking fallacy, that thought is economized by dispensing with it altogether. I prefer the saying of Clifford, with reference to $(a + b)^2 = a^2 + 2ab + b^2$ and its expression in English: "Two things may be observed on this comparison, First, how very much the shorthand expression gains in clearness from its brevity. Secondly, that it is only shorthand for something which is just straightforward common sense and nothing else. We may always depend upon it that algebra, which cannot be translated into good English and sound common sense is bad algebra."

In his statement of the fundamental principles of algebra, Whitehead follows Grassmann to a large extent. He divides them into two classes, the general and the special; the former apply to the whole of ordinary and universal algebra, the latter apply to special branches only. The general principles are as follows : Addition follows the commutative and associative laws; multiplication follows the distributive law, but does not necessarily follow the commutative and associative laws. The theory looks beautiful and plausible, but it does not stand the test of comparison with actual analysis, for quaternions is one of the principal branches of universal algebra, and in it the addition of indices is in general non-commutative, and the power of a binomial of indices is not formed after the distributive law.

But in addition to this formal bond we find in the book another bond uniting the several parts into one whole. In the preface Mr. Whitehead says, " The idea of a generalized conception of space has been made prominent in the belief that the properties and operations involved in it can be made to form a uniform method of interpretation of the various alge-

bras. Thus it is hoped in this work to exhibit the algebras both as systems of symbolism, and also as engines for the investigation of the possibilities of thought and reasoning connected with the abstract general idea of space." The chance for any arbitrary system of symbolism applying to anything real is very small, as the author admits; for he says that the entities created by conventional definitions must have properties which bear some affinity to the properties of existing things. Unless the affinity or correspondence is perfect, how can the one apply to the other? How can this perfect correspondence be secured, except by the conventions being real definitions, the equations true propositions, and the rules expressions of universal properties? The placing of the algebra of logic on a space basis has been criticised, but in reply it may be pointed out that logicians have been accustomed ever since the time of Euler to prove their principal theorems by means of diagrams.

Our conclusion about the fundamental rules of algebra is: If the elements of a sum or a product are independent of order, then the written order of the terms is indifferent, and the product of two such sums is the sum of the partial products; but when the elements of a sum or of a product have a real order, then the written order of the elements must be preserved though the manner of their association may be indifferent, and a power of a binomial is then different from a product. This applies whether the sum or product occurs simply, or as the index of a base.

Descartes wedded algebra to geometry; formalism tends to divorce them. The progress of mathematics within the century has been from formalism towards realism; and in the coming century, it may be predicted, symbolism will more and more give place to notation, conventions to principles, and loose extensions to rigorous generalizations.

REPORTS READD.

REPORT ON PROGRESS IN NON-EUCLIDEAN GEOMETRY. BY PROF. GEORGE BRUCE HALSTED, University of Texas, Austin, Texas.

It marks an epoch in the history of mathematics, that at a meeting of a great Association for the Advancement of Science, there should be presented by invitation a Report on Non-Euclidean Geometry.

Its two creators, Lobachévski, who misnamed it 'imaginary geometry,' and Bolyai János, under the nobler name 'science absolute of space,' failed utterly, while they lived, to win any appreciative attention for what is to-day justly honored as one of the profoundest advances of all time. The only recognition, the only praise of the achievement of Lobachévski ever printed in his lifetime was by Bolyai Farkas, the father of his brilliant young rival, and appeared in a little book with no author's name on the title page, and which we have no evidence that Lobachévski ever saw, a book so rare that my copy is probably the only one on the western continent.

When after more than forty years their names were rescued from oblivion by Baltzer and Hoüel, in 1866, still envious Time gave them back only with an aspersion against the genuineness of their originality. A cruel legend tarnished still their fame so long delayed, so splendidly deserved.

Even when their creation had reached the high dignity of being made the subject of courses of lectures for consecutive semesters at the University of Göttingen, yet on page 175 of the second impression of these lectures, 1893, we still find Felix Klein saying:

"Kein Zweifel bestehen kann, dass Lobatcheffsky sowohl wie Bolyai die Fragestellung ihrer Untersuchungen der Gaussischen Anregung verdanken."

It is a pleasant privilege to begin my report by announcing the rigorous demonstration that this ungenerous legend is untrue.

This point need not further delay us, since it has been treated by me at length in *Science*, N. S, Vol. IX, No. 232, pages 813–817, June 9, 1899.

What a contrast to the pathetic neglect of its creators, Lobachévski dying blind, unrecognized, without a single follower, Bolyai János dying of disgust with himself and the world, lies in the fact that less than a year ago our American magazine, the *Monist*, secured from the famous Poincaré, at great cost, a brilliant contribution to this now universally interesting subject, which I had the honor, through my friend, T. J. McCormack, of reading in the original French manuscript.

This extraordinary paper, published only in English translation, appears in the *Monist*, Vol. 9, No. 1, October, 1898, pp. 1-43.

In the first section of his greatest work, Lobachévski says :

"*Juxtaposition* (contact) is the distinctive characteristic of solids, and they owe to it the name, *geometric solids*, when we retain this attribute, taking into consideration no others, whether essential or accidental.

" Besides bodies, for example, also time, force, velocity, are the object of our judgment ; but the idea contained in the word juxtaposition does not apply thereto. In our mind we attribute it only to solids, in speaking of their composition or dissection into parts.

" This simple idea, which we have received directly in nature through the senses, comes from no other, and consequently is subject to no further explanation.

"Two solids, A and B, touching one another, form a single geometric solid C, in which each of the component parts A, B appears separate without being lost in the whole C.

"Inversely, every solid C is divided into two parts, A and B, by any *section* S.

" By the word section we understand here no new attribute of the solid, but again a juxtaposition, expressing thus the partition of the solid into two juxtaposed parts. In this way we can represent to ourselves all solids in nature as parts of a single whole solid, which we call space."

Poincaré starts off somewhat differently. He says : " We at once perceive that our sensations vary, that our impressions are subject to change.

" The laws of these variations were the cause of our creating geometry and the notion of geometrical space.

" Among the changes which our impressions undergo, we distinguish two classes :

" (1) The first are independent of our will and not accompanied by muscular sensations. These are *external changes* so-called.

" (2) The others are voluntary and accompanied by muscular sensations. We may call these *internal changes*.

" We observe next that in certain cases, when an external change has modified our impressions, we can, by voluntarily provoking an internal change, re-establish our primitive impressions.

" The external change, accordingly, can be *corrected* by an internal change.

" External changes may, consequently, be sub-divided into the two following classes :

" 1. Changes which are susceptible of being corrected by an internal change. These are *displacements*.

" 2. Changes which are not so susceptible. These are *alterations*.

" An immovable being would be incapable of making this distinction. *Such a being, therefore, could never create geometry*,—even if his sensations were variable, and even if the objects surrounding him were movable."

How like what Lobachévski said more than sixty years before : " We

cognize directly in nature only motion, without which the impressions our senses receive are not possible. Consequently all remaining ideas, for example geometric, are created artificially by our mind, since they are taken from the properties of motion ; and therefore, space in itself, for itself alone, does not exist for us."

Poincaré continues : " *the aggregate of displacements is a group.*"

At once rise before us the great names Riemann, Helmholtz, Sophus Lie.

In fact Poincaré's next section is merely a restatement of part of Riemann's marvellous address, published 1867, on the hypotheses at the basis of geometry.

Again, though the work of Helmholtz did not contain the group idea, yet it had put the problem of non-Euclidean geometry into the very form for the instrument of Sophus Lie, who calls it the 'Riemann-Helmholtz space-problem.'

To the genius of Helmholtz is due the conception of studying the essential characteristics of a space by a consideration of the movements possible therein.

Felix Klein it was, who first called the attention of Lie to this work of Helmholtz, before that unknown to Lie, and pointed out its connection with Lie's Theory of Transformationgroups, inciting him to a group-theory investigation of the problem.

In 1886 Lie gave briefly his weightiest results in a note : "Bemerkungen zu v. Helmholtz' Arbeit ueber die Thatsachen, die der Geometrie zu Grunde liegen," in the *Berichte of the Saxon Academy*, Leipzig, where in 1890 he gave his completed work in two papers, "Ueber die Grundlagen der Geometrie" (pp. 284-321, 355-418).

The whole investigation, published in volume III of his "Theorie der Transformationsgruppen," 1893, was in 1897 awarded the first Lobachévski prize.

Felix Klein declared that it excels all comparable works so absolutely that a doubt about the award could scarcely be possible.

Lie gives two solutions of the problem. In the first he investigates in space a group possessing free mobility in the infinitesimal, in the sense, that if a point and any line-element through it be fixed, continuous motion shall still be possible ; but if besides any surface-element through the point and line-element be fixed, then shall no continuous motion be possible.

The groups in tri-dimensional space possessing in a real point of general position this free mobility, Lie finds to be precisely those characteristic of the Euclidean, and two non-Euclidean geometries.

Strangely enough, for the seemingly analogous and simpler case of the plane, or two-dimensional space, these are not the only groups.

There are others where the paths of the infinitesimal transformations are spirals.

Without the group idea, Helmholtz had reached this reality, and as a consequence concluded that also to characterize our tri-dimensional spaces a new condition, a new axiom, was needed, that of *monodromy*.

It is one of the most brilliant results of Lie's second solution of the space problem, that starting from transformation-equations with three of Helmholtz's four assumptions he proves that the fourth, the famous "Monodromie des Raumes," is, in space of three dimensions, wholly superfluous. What a demonstration of the tremendous power of Lie's Group Theory !

Lie's method in general, as it appears in the *Berichte*, is the following:

Consider a tri-dimensional space, in which a point is defined by three quantities x, y, z.

A movement is defined by three equations : $x_1 = f(x, y, z)$; $y_1 = \phi(x,y,z)$; $z_1 = \psi(x,y,z)$.

By this transformation an assemblage, A, of points (x, y, z) becomes an assemblage, A', of points $(x_1, y_1 z_1)$.

This represents a movement which changes A to A'. Now make, in regard to the space to be studied, the following assumption :

In reference to any pair of points which are moved, there is *something* which is left unchanged by the motion.

That is, after an assemblage of points, A, has been turned by a single motion into an assemblage of points, A', there is a certain function, Ω, of the coordinates of any pair of the old points (x_1, y_1, z_1), (x_2, y_2, z_2), which equals that same function, Ω, of the corresponding new coordinates $(x'_1, y'_1 z'_1)$, $(x'_2, y'_2 z'_2)$; that is

$$\Omega(x'_1, y_1, z'_1, x'_2, y'_2, z'_2) = \Omega(x_1, y_1, z_1, x_2, y_2, z_2).$$

This *something* corresponds to the generalized idea of distance interpreted as independent of measurement by superposition of an unchanging sect as unit for length.

Moreover assume:

If one point of the assemblage is fixed, every other point of this assemblage, *without any exception*, describes a surface (a two-dimensional aggregate). When two points are fixed, a point in general (exceptions being possible) describes a curve (a one-dimensional aggregate). Finally if three points are fixed, all are fixed (exceptions being possible).

Then Lie proves exhaustively that the group consists either of all motions of Euclidean space or of all motions of non-Euclidean space.

The result is a remarkable one, demonstrating that the group of Euclidean motions and the group of non-Euclidean motions are, in tri-dimensional space, the only groups in which exist in the strict sense of the word free mobility. Thus, free motion in the strict meaning of the word can happen in three, and only three, spaces, namely, the traditional or Euclidean space, and the spaces in which the group of movements possible is the projective group transforming into itself one or the other of the surfaces of the second degree $x^2 + y^2 + z^2 \pm 1 = 0$.

To the fundamental assumption which completely characterizes these three groups, Lie gives also this form :

"If any real point $y_1{}^0, y_2{}^0, y_3{}^0$, of general position is fixed, then all real points x_1, x_2, x_3, into which may still shift another real point $x_1{}^0, x_2{}^0, x_3{}^0$, satisfy a real equation of the form

$$W(y_1{}^0, y_2{}^0, y_3{}^0; x_1{}^0, x_2{}^0, x_3{}^0; x_1, x_2, x_3) = 0,$$

which is not fulfilled for $x_1 = y_1{}^\circ$, $x_2 = y_2{}^\circ$, $x_3 = y_3{}^\circ$, and which represents a real surface passing through the point $x_1{}^\circ$, $x_2{}^\circ$, $x_3{}^\circ$.

"About the point $y_1{}^\circ$, $y_2{}^\circ$, $y_3{}^\circ$, may be so demarcated a triply extended region, that on fixing the point $y_1{}^\circ$, $y_2{}^\circ$, $y_3{}^\circ$, every other real point $x_1{}^\circ$, $x_2{}^\circ$, $x_3{}^\circ$,, of the region can yet shift continuously into every other real point of the region, which satisfies the equation $W = 0$, and which is joined to the point $x_1{}^\circ$, $x_2{}^\circ$, $x_3{}^\circ$, by an irreducible continuous series of points."

It is a satisfaction to the world of science that Lie's vast achievements were recognized while he lived.

Poincaré accepts and expounds his doctrine, saying in the article already mentioned.

"The axioms are not analytical judgments *a priori*; they are conventions. * * * Thus our experiences would be equally compatable with the geometry of Euclid and with a geometry of Lobachévski which supposed the curvature of space to be very small. We choose the geometry of Euclid because it is the simplest.

"If our experiences should be considerably different, the geometry of Euclid would no longer suffice to represent them conveniently, and we should choose a different geometry."

When, on November 3, 1897, the great Lobachévski prize was awarded to Lie, three other works were given honorable mention.

The first of these is a thesis on non-Euclidean geometry, by M. L. Gérard, of Lyons.

Lovers of the non-Euclidean geometry are naturally purists in geometry, and keenly appreciate Euclid's using solely such figures as he has rigorously constructed.

They understand that problems of construction play an essential part in a scientific system of geometry. Far from being solely, as our popular text-books suppose, practical operations, available for the training of learners, they have in reality, as Helmholtz declares, the force of existential propositions. Therefore is evident the high import of Gérard's work to establish the fundamental propositions of non-Euclidean geometry without hypothetical constructions other than the two assumed by Euclid: (1). Through any two points a straight can be drawn. (2). A circle may be described from any given point as center with any given sect as radius. Gérard adds explicitly the two assumptions: (3). A straight which intersects the perimeter of a polygon in a point other than one of its vertices intersects it again. (4). Two straights, or two circles, or a straight and a circle, intersect if there are points of one on both sides of the other.

Upon these four hypotheses, perfecting a brilliant idea of Battaglini (1867), Gérard establishes the relations between the elements of a triangle.

Lobachévski never explicitly treats the old problems changed by transference into the new geometric world, such as " Through a given point to draw a parallel to a given straight;" nor yet the seemingly impossible

problems now in it capable of geometric solution, such as "To draw to one side of an acute angle the perpendicular parallel to the other side ;" "To square the circle."

These would be sought in vain in the two quarto volumes of Lobachévski's collected works.

Bolyai János, in his all too brief two dozen pages, gives solutions of them, startling in their elegance.

But in establishing his theory, he uses, for the sake of conciseness, the principle of continuity even more freely than does Lobachévski.

Gérard, in the second part of his memoir, gives the elements of non-Euclidean analytic geometry, and in the third part a strict treatment of equivalence.

Even Euclid, in proving his I. 35, "Parallelograms on the same base, and between the same parallels, are equal to one another," does not show that the parallelograms can be divided into pairs of pieces admitting of superposition and coincidence. He uses rather the assumption explicitly set forth by Lobachévski, "Two surfaces are equal when they are sums or differences of congruent pieces." But Cresswell, in his "Treatise of Geometry," showed how to cut the parallelograms into parts congruent in pairs.

The same can be done for Euclid I. 43 : "The complements of the parallelograms which are about the diagonal of any parallelogram are equal."

Hence we may use the definition : Magnitudes are equivalent which can be cut into parts congruent in pairs.

This method I applied to the ordinary Euclidean geometry in my "Elementary Synthetic Geometry" before the appearance of Gérard's work, where it is extended to the non-Euclidean.

Regarding the first assumed construction of Euclid and Gérard : "A straight can be drawn through any two points," W. Burnside has given us a charming little paper in the *Proceedings of the London Mathematical Society*, Vol. XXIX, pp. 125–132 (Dec. 9, 1897) entitled "The Construction of the Straight Line Joining Two Given Points."

Euclid's postulate implies the use of a ruler or straight-edge of any required finite length.

The postulate is clearly not intended to apply to the case in which the distance between the two points is infinite. In fact Euclid I. 31, gives a compass and ruler construction for the line when one of the points can be reached while the other cannot.

The other exceptional case, when neither point can be reached, *i. e.*, when the two given points are the points at infinity on two non-parallel lines, is not dealt with by Euclid.

In elliptic space any one point can be reached from any other by a finite number of finite operations. The line joining two given points can therefore be always constructed with the ruler alone.

In hyperbolic space, if we deal with projective geometry, we must assume that *every* two straight lines in a plane determine a point. When

the two straight lines are non-intersectors the point can neither be a finite point nor a point at infinity.

Such a point is termed an "ideal" point.

The problem of constructing the straight line joining two given points involves therefore three further cases; namely, (IV) that in which one of the points is a finite point and the other an ideal point; (V) that in which one is a point at infinity and the other an ideal point; (VI) that in which both points are ideal points.

It is a pleasure to signal the appearance, within the past year, of the second volume of the exceedingly valuable work of Dr. Wilhelm Killing, "Einfuehrung in die Grundlagen der Geometrie," (Paderborn, 1898).

With Killing's name will be associated the tremendous difference living geometers find between the properties of a finite region of space, and the laws which pertain to space as a whole.

Of the word *direction* he says, it can only be given a meaning when the theory of parallels is already presupposed.

The pseudo-proof of the parallel postulate still given in current text-books, for example by G. C. Edwards in 1895, Killing calls the Thibaut proof, saying that it has especial interest because its originator, who was professor of mathematics at Göttingen with Gauss, published the attempt at a time, 1818, when Gauss had already called attention to the failure of attempts to prove this postulate, and had declared that we had not progressed beyond where Euclid was two thousand years before.

But Killing is here in error when he supposes Thibaut the originator of this popular pseudo-proof.

It was given in 1813 by Playfair in his edition of Euclid, in a Note to I. 29.

It was very elegantly shown to be a fallacy by Colonel T. Perronet Thompson, of Queen's College, Cambridge, in a remarkable book called " Geometry without Axioms," of which the third edition is dated 1830, a book seemingly unknown in Germany, since Engel and Staeckel copy from Riccardi the title (with the mistake " first books " for " first book ") under the date 1833, which is the date of the fourth edition.

Killing has won an important place by investigating the question, what varieties of connection of space are compatible with the different elemental arcs of constant curvature.

Riemann, Helmholtz and Lie consider only a region of space, and give analytic expressions for the vicinity of a point.

If this region be extended, the question is, what kind of connection of space can result.

Killing shows there are different possibilities, really a series of topologically different forms of space with Euclidean, Lobachévskian, Riemannian geometry in the bounded, simply connected region.

The germinal idea is due to Clifford who in an unprinted address before the Bradford meeting of the British Association (1873) "On a surface of zero curvature and finite extent," and also by a remark in his paper "Preliminary sketch of biquaternions," called attention to a recurrent surface in single elliptic space, which has every-

where zero for measure of curvature, yet is nevertheless of only finite area.

Similarly complete universal spaces are found of zero or negative measure of curvature, which nevertheless are only of finite extent.

Since there is no way of proving that the whole of our actual space can be moved in itself in ∞^6 ways, it may possibly be, after all, one of these new Clifford spaces.

Free mobility of bodies may only exist while they do not surpass a certain size.

Killing devotes an interesting section, over seven pages, to Legendre's definition of the straight line as the shortest distance between two points. He emphasizes three principal reasons why this is inadmissible.

These are (a) since the possibility of measurement for all lines is presumed beforehand, which is not allowable; (b) since before the execution of the measurement there must be a measuring standard, but this is first given by the straight line; (c) since the existence of a minimum is not evident, on the contrary can be demanded only as an assumption.

The first objection was always conclusive, yet it strengthens every day, for our new mathematics knows of lines, real boundaries between two parts of a plane, to which the idea of length is inapplicable.

Under the title " Universal Algebra" one would scarcely look for a treatise on non-Euclidean geometry. Yet the first volume of Whitehead's admirable work (Cambridge, 1898, pp. 586) devotes more than 150 pages to an application of Grassmann's 'Calculus of Extension,' to hyperbolic, elliptic, parabolic spaces. So devoted is he that we find him saying : "Any generalization of our space conceptions, which does not at the same time generalize them into the more perfect forms of hyperbolic or elliptic geometry, is of comparatively slight interest."

He emphasizes the fact that the three-dimensional space of ordinary experience can never be proved parabolic. "The experience of our senses, which can never attain to measurements of absolute accuracy, although competent to determine that the space-constant of the space of ordinary experience is greater than some large value, yet cannot, from the nature of the case, prove that this space is absolutely Euclidean."

From the many important contributions by Whitehead may be singled out as especially timely his development of a theorem of Bolyai János to which F. S. Macaulay called especial attention in the second of his able articles entitled John Bolyai's " Science Absolute of Space" (*The Mathematical Gazette*, No. 8, July, 1896, pp. 25-31; No. 9, October, 1896, pp. 49-60). Macaulay says, p. 53. "Finally follows a theorem (§ 21), which is undoubtedly the most remarkable property of hyperbolic space, that the sum of the angles of any triangle formed by L-lines on an F-surface is equal to two right angles. On this theorem Bolyai remarks (Halsted's Bolyai, 4th Ed., p. 18): 'From this it is evident that Euclid's Axiom XI and all things which are claimed in geometry and plane trigonometry hold good *absolutely* in F, L-lines being substituted in place of straights.

Therefore the trigonometric functions are taken here in the same sense (are defined here to have the same values) as in Σ (as in Euclidean geometry); and the periphery of the circle, of which the L-form radius $= r$ in F, is $= 2\pi r$, and likewise the area of circle with radius r (in F) $= \pi r^2$ (by π understanding half the periphery of circle with radius 1 in F, or the known 3.1415926 . . .).'"

Whitehead in his Universal Algebra, § 262, recurs to this important point, saying, "The idea of a space of one type as a locus in space of another type, and of dimensions higher by one, is due partly to J. Bolyai, and partly to Beltrami.

Bolyai points out that the relations between lines formed by great circles on a two-dimensional limit-surface are the same as those of straight lines in a Euclidean plane of two dimensions.

"Beltrami proves, by the use of the pseudosphere, that a hyperbolic space of any number of dimensions can be considered as a locus in Euclidean space of higher dimensions.

" There is an error, popular even among mathematicians misled by a useful technical phraseology, that Euclidean space is in a special sense flat, and that this flatness is exemplified by the possibility of a Euclidean space containing surfaces with the properties of hyperbolic and elliptic spaces. But the text shows that this relation of hyperbolic to Euclidean space can be inverted. Thus no theory of the flatness of Euclidean space can be founded on it." Whitehead has since followed up his point in a very important and powerful paper in the *Proceedings of the London Mathemetical Society*, Vol. XXIX, pp. 275-324, March 10, 1898, entitled "The Geodesic Geometry of Surfaces in non-Euclidean Space." He there says, "The relations between the properties of geodesics on surfaces and non-Euclidean geometry, as far as they have hitherto been investigated, to my knowledge, are as follows :

" It has been proved by Beltrami that the 'geodesic geometry' of surfaces of constant curvature in *Euclidean* space is the same as the geometry of straight lines in planes in elliptic or in hyperbolic space, according as the curvature of the surface is positive or negative.

" The geometry of great circles on a sphere of radius ρ in elliptic space of " space-constant" γ is the same as the geometry of straight lines in planes in elliptic space of space constant $\gamma \sin \dfrac{\rho}{\gamma}$.

"The geometry of great circles on a sphere of radius ρ in hyperbolic space of " space-constant" γ is the same as the geometry of straight lines in planes in elliptic space of space-constant $\gamma \sin h\dfrac{\rho}{\gamma}$.

" The geometry of geodesics (that is, lines of equal distance), on a surface of equal distance, σ, from a plane in hyperbolic space of space-constant γ, is the same as that of straight lines in planes in hyperbolic space of space-constant $\gamma \cos h \dfrac{\sigma}{\gamma}$.

" Finally, the geometry of geodesics (that is, limit-lines), on a limit-

surface in hyperbolic space—which may be conceived either as a sphere of infinite radius or as a surface of equal, but infinite, distance from a plane—is the same as that of straight lines in planes in Euclidean space.

" The preceding propositions are due directly, or almost directly, to John Bolyai, though, of course, he only directly treats of hyperbolic space.

" From the popularization of Beltrami's results by Helmholtz, and from the unfortunate adoption of the name 'radius of space curvature' for γ (here called the space-constant), many philosophers, and, it may be suspected from their language, many mathematicians, have been misled into the belief that some peculiar property of flatness is to be ascribed to Euclidean space, in that planes of other sorts of space can be represented as surfaces in it.

" This idea is sufficiently refuted, at least as regards hyperbolic space, by Bolyai's theorem respecting the geodesic geometry of limit-surfaces. For a Euclidean plane can thereby be represented by a surface in hyperbolic space.

" It is the object of this paper to extend and complete Bolyai's theorem by investigating the properties of the general class of surfaces in any non-Euclidean space, elliptic or hyperbolic, which are such that their geodesic geometry is that of straight lines in a Euclidean plane.

" Such surfaces are proved to be real in elliptic as well as in hyperbolic space, and their general equations are found for the case when they are surfaces of revolution.

" In hyperbolic space, Bolyai's limit-surfaces are shown to be a particular case of such surfaces of revolution. The surfaces fall into two main types : the limit-surfaces form a transition case between these types.

In elliptic space there is only one type of such a surface of revolution.

" The same principles would enable the problem to be solved of the discovery in any kind of space of surfaces with their 'geodesic' geometry identical with that of planes in any other kind of space."

So that which Macaulay designated as "undoubtedly the most remarkable property of hyperbolic space," has been by Whitehead not only generalized for hyperbolic space but extended to elliptic space.

Bolyai János seemed fully to realize the weight, the scope, the possibilities, the meaning of his discovery. He returns to it in §37, where he uses the proportionality of similar triangles in F to solve an essential problem in S (hyperbolic space). Then he adds : " Hence easily appears (L-lines being given by their *extremities alone*), also fourth and mean *terms* of a proportion can be found, and all geometric constructions which are made in Σ in plano, in this mode can be accomplished in F, *apart from Axiom XI.*"

The italics are Bolyai's, yet I find that they have not been reproduced in my published translation (the only one in English), nor in Frischauf's German, nor in Hoüel's French, nor in Fr. Schmidt's Latin text, nor in Suták's Magyar.

Whitehead's researches will remind us all how great a thing it was to have reached the whole Euclidean system entirely apart from any parallel postulate.

It is a pleasure to be able to state that this was also done by Loba-chévski.

It is explicitly given in his first published work "O nachalah geo-metri" (1829); "Noviya nachala geometri" (1835), devotes to it Chapter VIII.

It is also at this point, so striking as pure mathematics, that general philosophy finds itself involved. Killing, Klein, and in general the German writers, distinctly draw back from any philosophical implications. The whole matter, however, has been ably opened in "An Essay on the Foundations of Geometry," by the Hon. Bertrand A. W. Russell, Fellow of Trinity College, Cambridge, (1897), who has had the good fortune to be the very first to set forth the philosophical importance of von Staudt's pure projective geometry, which in its foundation and dealing with the qualitative properties of space involves no reference to quantity.

I discussed this point more than twenty years ago in the *Popular Science Monthly*, à *propos* of Spencer's classification of the Abstract Sciences.

In a note to the first edition of his classification of the sciences (omitted in the second edition) Spencer says, "I was ignorant of the existence of this as a separate division of mathematics, until it was described to me by Mr. Hirst. It was only when seeking to affiliate and define 'Descriptive Geometry' that I reached the conclusion that there is a negatively quantitative mathematics as well as a positively quantitative mathematics."

As explanatory of what he wishes to mean by negatively quantitative we quote from his Table I :

"Laws of relations, that are quantitative (mathematics), negatively : the terms of the relations being definitely related sets of positions in space, and the facts predicated being the absence of certain quantities ('Geometry of Position')." He also says : "In explanation of the term 'negatively-quantitative,' it will be sufficient to instance the proposition that certain three lines will meet in a point, as a negatively-quantitative proposition, since it asserts the absence of any quantity of space between their intersections. Similarly, the assertion that certain three points would always fall in a straight line is 'negatively-quantitative,' since the conception of a straight line implies the negation of any lateral quantity or deviation."

But Sylvester has said of this very proposition that it "refers solely to position, and neither invokes nor involves the idea of quantity or magnitude."

"Projective geometry proper," says Russell, "does not employ the conception of magnitude."

Now it is in metrical properties alone that non-Euclidean and Euclidean spaces differ.

The distinction between Euclidean and non-Euclidean Geometries, so important in metrical investigations, disappears in projective geometry proper.

Therefore, projective geometry deals with a wider conception, a concep-

tion which includes both, and neglects the attributes in which they differ. This conception Mr. Russell calls "a form of externality." It follows that the assumptions of projective geometry must be the simplest expression of the indispensable requisites of all geometrical reasoning.

Any two points uniquely determine a line, the *straight*.

But any two points and their straight are, in pure projective geometry, utterly indistinguishable from any other point-pair and their straight. It is of the essence of *metric* geometry that two points shall completely determine a spatial *quantity*, *the sect* (German, Strecke). If Mr. Russell had used for this fundamental spatial magnitude this name, or any name but 'distance,' his exposition would have gained wonderfully in clearness.

It is a misfortune to use the already overworked and often misused word 'distance' as a confounding and confusing designation for a sect itself and also the measures of that sect, whether by superposition, ordinary ratio, indeterminate as depending on the choice of a unit, or projective metrics, indeterminate as depending on the fixing of the two points to be taken as constant in the varying cross ratios.

That Mr. Russell's Chapter, "A short history of metageometry," contains all the stock errors in particularly irritating form, and some others peculiarly grotesque, I have pointed out in extenso in *Science*, Vol. VI., pp. 487-491. Nevertheless the book is epoch making. It finds "that projective geometry, which has no reference to quantity, is necessarily true of any form of externality." "In metrical geometry is an empirical element, arising out of the alternatives of Euclidean and non-Euclidean space."

One of the most pleasing aspects of the universal permanent progress in all things non-Euclidean is the making accessible of the original masterpieces.

The marvellous ' *Tentamen* ' of Bolyai Farkas, as Appendix to which the 'Science Absolute' of Bolyai János appeared, a book so rare that except my own two copies I know of no copy on the western continent, a book which has never been translated, a field which has lain fallow for sixty-five years, is now being reissued in sumptuous quarto form by the Hungarian Academy of Sciences. The first volume appeared in 1897, edited with sixty-three pages of notes in Latin, by König and Réthy of Budapest.

Professor Réthy, whom I had the pleasure of meeting in Kolozsvár, tells me the second volume is in press, and he is working on it this summer.

Bolyai Farkas is the forerunner of Helmholtz, Riemann, Lie, though one would scarcely suspect it from the poetic exaltation with which he begins his great work.

" *Lectori salutem!*

" Scarce superficially imbued with the rudiments of first principles, of my own accord, without any other end, but led by internal thirst for truth, seeking its very fount, as yet a beardless youth I laid the foundations of this ' Tentamen.'

" Only fundamental principles is it proposed here so to present, that Tyros, to whom it is not given to cross on light wings the abyss, and, pure spirits, glad of no original, to be borne up in airs scarce respirable, may, proceeding with firmer step, attain to the heights.

" You may have pronounced this a thankless task, since lofty genius, above the windings of the valleys, steps by the Alpine peaks; but truly everywhere are present gordian knots needing swords of giants. Nor for these was this written.

" Forsooth I wish the youth by my example warned, lest having attacked the labor of six thousand years, alone, they wear away life in seeking now what long was found.

" Gratefully learn first what predecessors teach, and after forethought build. Whatever of good comes, is antecedent term of an infinite series."

His analysis of space starts with the principle of continuity: spatium est quantitas, est continuum (p. 442). This Euclid had used unconsciously, or at least without specific mention ; Riemann and Helmholtz consciously.

Second comes what he calls the *axiom of congruence*, p. 444. § 3: "Corpus idem in alio quoque loco videnti, quaestio succurrit : *num loca ejusdem diversa aequalia sint?* Intuitus ostendit, aequalia esse."

Riemann : "Setzt man voraus, dass die Koerper unabhaengig vom Ort existieren, so ist das Kruemmungsmass ueberall constant." See also the second hypothesis of Helmholtz.

Third, any point may be moved into any other; the free mobility of rigid bodies.

If any point remains at rest any region in which it is may be moved about it in innumerable ways, and so that any point other than the one at rest may recur. If two points are fixed motion is still possible in a specific way.

Three fixed points not costraight prevent all motion (p. 446, § 5).

Thus we have the third assumption of Helmholtz, combined with his celebrated Principle of Monodromy.

Bolyai Farkas deduces from these assumptions not only Euclid but the non-Euclidean systems of his son János, referring to the approximate measurements of astronomy as showing that the parallel postulate is not sufficiently in error to interfere with practice (p. 489). This is just what Riemann and Helmholtz afterward did, only by casting off also the assumption of the infinity of space they got also as a possibility for the universe an elliptic geometry, the existence of a case of which independently of parallels was first proven by Bolyai János when he proved spherics independent of Euclid's assumption.

So if Sophus Lie had ever seen the 'Tentamen,' he might have called his great investigation the ' Bolyai Farkas Space Problem ' instead of the Riemann-Helmholtz Space Problem.'

The first volume of the 'Tentamen' as issued by the Hungarian Academy does not contain the famous 'Appendix.' But in 1897 Franz Schmidt, that heroic figure, ever the bridge between János and the world, issued at Budapest the Latin text of the 'Science Absolute,' with a

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biography of Bolyai János in Magyar, and a Magyar translation of the text by Suták József.

Strangely enough, though the 'Appendix' had been translated into German, French, Italian, English, and even appeared in Japan, yet no Hungarian rendering had ever appeared.

It was Franz Schmidt who placed the monument over the forgotten grave of János, only identified because there still lived a woman who had loved him.

Now in this Magyar edition he rears a second monument. The introduction by Suták is particularly able.

The Russians have honored themselves by the great Lobachévski Prize. Why does not that glorious race, the Magyars, do tardy justice to their own genius in a great Bolyai Prize?

One other noble thing the Hungarian Academy of Sciences has just achieved, the publication in splendid quarto form of the correspondence between Gauss and Bolyai Farkas: (Briefwechsel zwischen Carl Friedrich Gauss und Wolfgang Bolyai).

It was again Franz Schmidt who after long endeavors at last obtained this correspondence from the Royal Society of Sciences at Göttingen, where Bolyai had sent the letters of Gauss at his death.

The correspondence is fitly edited by Schmidt and Staeckel.

It gives us a romance of pure science.

Gauss was the greater mathematician ; Bolyai the nobler soul and truer friend. On April 10, 1816, Bolyai wrote to Gauss giving a detailed account of his son János, then fourteen years old; and unfolding a plan to send János in two years to Göttingen, to study under Gauss.

He asks if Gauss will take János into his house, of course for the usual remuneration, and what János shall study meanwhile.

Gauss never answered this beautiful and pregnant letter, and never wrote again for sixteen years!

Had Gauss answered that letter, Göttingen might now perhaps have to boast a greater than Gauss, for in sheer genius, in magnificent nerve, Bolyai János was unsurpassable, as absolute as his Science of Space.

But instead, he joined the Austrian army, and the mighty genius which should have enriched the transactions of the greatest of learned societies with discovery after discovery in accelerating quickness, preyed instead upon itself, printing nothing but a brief two dozen pages.

Almost to accident the world owes the admirable volumes in which Staeckel and Engel contribute such priceless treasures to the non-Euclidean geometry.

An Italian Jesuit, P. Manganotti, discovered that one of his order, the Italian Jesuit Saccheri, had already in 1733 published a series of theorems which the world had been ascribing to Bolyai.

Thereupon in 1889 E. Beltrami published in the *Atti della Reale Accademia dei Lincei*, Serie 4, Vol. V., pp. 441-448, a note entitled "Un precursore italiano di Legendre e di Lobatschewski," giving extracts from Saccheri's book which abundantly proved the claim of Manganotti.

In the same year, 1889, E. d'Ovidio, in the *Torino Atti*, XXIV, pp. 512-513, called attention to this note in another entitled Cenno sulla Nota del prof. E. Beltrami : "Un precursore, etc.," expressing the wish that P. Manganotti would by a more ample discussion rescue Saccheri's work from unmerited oblivion.

Staeckel says the thought then came to him, whether Saccheri's work were not a link in a chain of evolution, the genesis of the non-Euclidean geometry.

In 1893 at the International Mathematical Congress at Chicago, in the discussion which followed my lecture "Some Salient Points in the History of non-Euclidean and hyper-spaces" wherein I gave an account of Saccheri with description of his book and extracts from it, Professor Klein, who had never before heard of Saccheri, and Professor Study of Marburg mentioned that there had recently been brought to light an old paper of Lambert's anticipating in points the non-Euclidean geometry, and named in this connection Dr. Staeckel.

I at once wrote to him and published in the *Bulletin of the New York Mathematical Society*, Vol. III., pp. 79-80, 1893, a note on Lambert's non-Euclidean geometry, mentioning Staeckel's purpose to republish Lambert's paper in the Abhandlungen of the Leipziger Gesellschaft der Wissenschaften. But after this, in January, 1894, Staeckel formed the plan to make of Saccheri and Lambert a book, and associating with him his friend Friedrich Engel, they gave the world in 1895 " Die Theorie der Parallellinien, eine Urkundensammlung zur Vorgeschichte der nichteuklidischen Geometrie."

Strengthened by the universal success of this book, they planned two volumes in continuation. Staeckel takes the volume devoted to Bolyai János and his father.

It is to begin with a more complete life of the two than has yet appeared, of course from material furnished largely by Franz Schmidt.

Then follows the " theoria parallelarum" of Bolyai Farkas, interesting as proving that in 1804, Gauss was still under the spell of Euclid. Then is to follow the Latin text of the immortal appendix with a German translation. Next comes in German translation selections from the 'Tentamen.' The book concludes with the geometric part of " Kurzer Grundriss," the only one of the Bolyai's works printed originally in German.

This volume in nearly finished and may be expected in a few weeks.

The volume undertaken by Engel has just appeared (1899). It is a German translation of Lobachévski's first published paper (1829), "On the principles of geometry," and also of his greatest work, "New elements of geometry, with complete theory of parallels." Only from the ' New Elements' can any adequate idea be obtained of the height, the breadth, the depth of Lobachévski's achievement in the new universe of his own creation.

Of equal importance is the fact that Engel's book gives to the world at

last a complete, available text-book of non-Euclidean geometry. There is no other to compare with it.

.For the history of non-Euclidean geometry we have the admirable Chapter X, of Loria's pregnant work "Il passato ed il presente delle principali teorie geometriche." This chapter cites about 80 authors, mostly of writings devoted to non-Euclidean geometry.

In my own "Bibliography of hyper-space and non-Euclidean Geometry," in the *American Journal of Mathematics* (1878), I gave 81 authors and 174 titles.

This when reprinted in the collected works of Lobachévski (Kazan, 1886) gives 124 authors, and 272 titles.

Roberto Bonola has just given in the *Bollettino di Bibliografia e Storia della Scienze Matematiche*, (1899), an exceedingly rich and valuable "Bibliografia sui Fondamenti della Geometria in relazione alla Geometria Non-Euclidea" in which he gives 353 titles.

This extraordinary output of human thought has henceforth to be reckoned with. Hereafter no one may neglect it who attempts to treat of fundamentals in geometry or philosophy.

REPORT ON THE RECENT PROGRESS IN THE THEORY OF LINEAR GROUPS. BY PROFESSOR L. E. DICKSON, University of California, Berkeley, Cal.

[To be published in the Bulletin of the American Mathematical Society.]

The report was of the nature of a supplement to the report on finite groups read at the last annual meeting of the Association by Dr. G. A. Miller, of Cornell. It is intended for publication in the Bulletin of the American Mathematical Society, in which the report of Dr. Miller appeared last year.

Part I of the present report gives the general theorems relating to the canonical form of finite groups of linear substitution and to the generators of such groups. After a complete enumeration of the binary and ternary collineations in their historical setting, a number of special quaternary linear groups, particularly the famous one of order 51840, are considered.

Part II treats of linear groups in a Galois Field, their order, generators, factors of composition and the isomorphisms existing between them. In § 3 the Galois Field is defined and a full bibliography is added. In § 4–13 are treated in turn the general linear homogeneous group, the linear fractional group, the Abelian linear group and its generalized form, the first and second hypoabelian groups, the orthogonal group, other linear groups with a quadratic invariant or a special invariant of degree q, the hyperorthogonal group, and the hyperabelian group. In § 14 a number of isomorphisms existing between these groups are tabulated. As many as six forms of a simple group of order 25920 are given, this group having applications in various geometric problems.

PAPERS READ.

[TITLES AND ABSTRACTS.]

THE ABERRATION CONSTANT FROM OBSERVATIONS OF POLARIS. BY PROF. ASAPH HALL, JR., University of Michigan.

Observations have been made by Prof. Hall since about May 1, 1898, of meridian zenith distances of Polaris with the idea of determining the latitude variation at Ann Arbor and the aberration constant.

The observations were made above and below the pole, direct and reflected. The direct observations up to July, 1899, give for the upper and lower culmination respectively :

Aberration constant	20."60
Parallax	0."32
Aberration constant	20."58
Parallax	0."29

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ANCIENT ECLIPSES AND CHRONOLOGY. BY R. W. McFARLAND, Oxford, O.

SOME POINTS IN THE DESIGN OF A SPECTROSCOPE. BY PROF. H. C. LORD, Ohio State University.

NOTE ON GRASSMANN'S PROOF THAT THERE CAN BE BUT TWO KINDS OF LINEAL MULTIPLICATION OF TWO FACTORS. BY JOS. V. COLLINS, PH.D., State Normal School, Stevens Point, Wis.

The proof of this theorem, the fundamental one in Grassmann's system, is long and rather difficult to follow. It virtually deals with the problem of simplifying a distributed product whose units are not those of ordinary analysis. It may be remarked that it was while studying this same question that Hamilton came upon his quaternions. Macfarlane in one place makes the interesting remark that the holding of the distributive law seems to be fundamental in multiplication, different in this respect from the associative and commutative laws.

One part of Grassmann's demonstration can be shortened, and to show how is the object of this paper.

Grassmann starts with an assumed law of simplification expressed by the formula

$$(a) \qquad \sum a_{rs} [a_r a_s] = 0$$

in which he substitutes for the units a_r and a_s the following values respectively, $\sum x_{ru} E_u$ and $\sum x_{sv} E_v$, getting

$$\sum x_{ru} x_{sv} a_{rs} [E_u E_v] = 0.$$

From this equation by symmetry he obtained as the condition that the assumed law of simplification will be satisfied when distributed values are substituted for its units (defined as a *lineal* multiplication), the equation

$$(b) \qquad \sum x_{ru} x_{sv} \{ a_{rs}[E_u E_v] + a_{sr}[E_v E_u] \} = 0.$$

This equation, in the nature of the case, must hold true whatever arbitrary values are substituted for the x's. Instead of letting, as Grassmannn does, a particular x_{ru} take the values 1 and -1, let it take values $2x_{r'u'}$ and $x_{r'u'}$. Substituting these in (b), and subtracting the latter equation from the former, we get, since all the terms which do not contain $x_{r'u'}$ cancel out,

$$x_{r'u'} \sum x_{sv} \{ a_{r's}[E_{u'} E_v] + a_{sr'}[E_v E_{u'}] + 3x_{r'u'} a_{r'r'}[E_{u'} E_{u'}] \} = 0$$

in which $s = r'$ and $v = u'$ is excluded in the summing. The last equation may be written

$$\sum x_{sv} \{ a_{r's}[E_{u'} E_v] + a_{sr'}[E_v E_{u'}] \} + 2x_{r'u'} a_{r'r'}[E_{u'} E_{u'}] = 0.$$

Let a particular coefficient $x_{s'v'}$ in the last equation take in succession the values 2 and 1, and subtract the latter equation from the former. This gives

$$(c) \qquad a_{r's'}[E_{u'} E_{v'}] + a_{s'r'}[E_{v'} E_{u'}] = 0,$$

which by the nature of the argument must be true for all values of r, s, u and v. Putting $u' = v'$ in (c) and setting in turn each factor of the resulting equation equal to zero, Grassmann gets for the four possible kinds of lineal multiplications:

(1) That whose simplication law is $[E_u E_v] = [E_v E_u]$

(2) " " " " " $[E_u E_v] = -[E_v E_u]$

giving the combinatory multiplication of which so much is made.

(3) That in which all the coefficients are zero.

(4) That in which there is no attempt at simplication.

Recurring to the demonstration it is easy to see that (c) can be inferred directly from (b) provided we say that if the x's are to be allowed to have any values their products in pairs may have any values. The above demonstration, as also Grassmann's, avoids introducing this principle.

The derivation of (b) from the equation which immediately precedes it above is extraordinarily simple. If the terms of the first equation be written out and arranged, one gets two types of terms, *viz.*,

$$x^2_{ru} a_{rr} [E_u E_u] \text{ and } x_{ru} x_{sv} \{ a_{rs} [E_u E_v] + a_{sr}[E_v E_u] \}.$$

After multiplying the equation in its expanded form through by 2, the new equation can be written in the form (b), since

$$2\,a_{rr}\,[\,E_u\,E_u\,] = a_{rr}\,[\,E_u\,E_u\,] + a_{rr}\,[\,E_u\,E_u\,].$$

Thus (b) is a *necessary* instead of, as at first appears, an abitrary inference from the preceding equation.

THE THEORY OF MATHEMATICAL INFERENCE. BY GEORGE J. STOKES, Professor in Queens College, Cork.

The author distinguishes three theories of mathematical inference. 1. The verbal or nominalist theory represented by James Mills. 2. The empirical theory in which the synthetic element in mathematical inference is derived from experience. 3. The Kantian theory of *a priori* synthesis. In opposition to these views the theory is advocated that the fundamental truths of mathematics are logical consequences of the mere fact or possibility of synthesis generally, and that ordinary mathematical inference is compounded of a logical or analytical element which has been reduced to mathematical form in Boole's " Laws of Thought" and a synthetic element represented by algebras of the type of Grassmann's "Ausdehnungslehre." The author seeks to confirm the above theory by the analysis already put forward by him of the imaginary of ordinary mathematics into the conjunctive combination of the signs + and —, an analysis suggested by certain principles of Boole and Schroeder. The theory is further confirmed by reference to the views put forward in a memoir on the Theory of Mathematical Form in the Philosophical Transactions of the Royal Society by Mr. A. B. Kempe, F.R.S., and in subsequent papers by the same author.

PRACTICAL ASTRONOMY DURING THE FIRST HALF OF THE PRESENT CENTURY. BY PROF. T. H. SAFFORD, Williamstown, Mass.

ON THE COMMUTATORS OF A GROUP. BY G. A. MILLER, Cornell University, Ithaca, N. Y.

Let $s_1, s_2, s_3, \ldots, s_K$ represent all the operators of a group (G), and let t be any operator whatever. From the identity

$$s_\beta^{-1}\,s_\alpha^{-1}\,t^{-1}\,s_\alpha\,t\,s_\beta = (s_\alpha\,s_\beta)^{-1}\,t^{-1}\,s_\alpha\,s_\beta\,t.\ t^{-1}s_\beta^{-1}t\,s_\beta$$

we observe that the transform with respect to the operators of G of any commutator formed with t and any operator of G is the product of two such commutators. These commutators must therefore form a group which is transformed into itself by all the operators of G. When t trans-

forms G into itself, the given commutators generate the smallest selfcon-jugate subgroup of G, which has the property that all the operators of the corresponding quotient group are commutative to t.

If t remains fixed in the commutator $C \equiv s^{-1}t^{-1}st$ while s is multiplied on the left by all the operators of G we observe that C remains unchanged when this multiplier is commutative to t, and that it is changed for every other multiplier. Arranging all the operators of G in lines, in the usual manner, the first line containing all the operators that are commutative to t, we observe that C has as many distinct values as there are lines. Hence C has as many different values as t has conjugates when it is transformed by all the operators of G.

LINEAR VECTOR FUNCTIONS.   BY S. KIMURA, Japan.

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ADDRESS

BY

ELIHU THOMSON,

VICE-PRESIDENT, AND CHAIRMAN OF SECTION B.

THE FIELD OF EXPERIMENTAL RESEARCH.

Physical research by experimental methods is both a broadening and a narrowing field. There are many gaps yet to be filled, data to be accumulated, measurements to be made with great precision, but the limits within which we must work are becoming, at the same time, more and more defined.

The upper ranges of velocities, temperatures, and pressures, which manifest themselves in the study of the stellar universe, are forever beyond the range of experiment. But while the astronomer must wait for opportunities to observe, the experimenter can control his conditions and apply his methods and his apparatus at once to the question in hand. Still this work must be done within a certain range or must be limited to conditions more or less easy to recognize. In spite of this fact, however, the progress made during the past century is not likely to cease or abate in the next, and the ever-increasing number of workers bodes well for the future enrichment of our science.

Whatever may be our ideas of fundamental entities, as expressed in various theories; whether, as an example, we regard the ether as like an infinitely mobile fluid, or as an incompressible solid, or as a jelly; or whether we incline to

think that being an electromagnetic medium, it may be without mechanical properties, which properties depend in some way upon the electromagnetic nature of the ether; we cannot reach sure ground without the experimental test.

The development in the field of research by experiment is like the opening of a mine, which, as it deepens and widens, continually yields new treasure, but with increased difficulty, except when a rich vein is struck and worked for a time. In general, however, as the work progresses there will be needed closer application and more refined methods. We may indeed find our limit of depth in the mine of experiment in inordinate cost, in temperatures too high, or in pressures beyond the limits of our skill to control.

It is but a few months since Prof. Dewar, by the evaporation of liquid hydrogen in a vacuum, closely approached, if he has not reached, our lower limit of possible temperature. Investigations of the effects of low temperature upon the properties of bodies must from the present outlook be forever limited to about 20° C. above absolute zero ; unless, a lighter gas than hydrogen be discovered upon the earth, the actual existence of which it is, of course, impossible to conjecture. Before the actual experimental demonstration of this limit, the limit itself was known to theory, at least approximately, but the spur of the experimenter is the overcoming of difficulties and the possibility of new discoveries which come as surprises. In the case in question a liquid of extremely low density, only one-fourteenth that of liquid nitrogen, was produced, while still defined by clear and well-marked refracting surfaces.

When we turn to the consideration of the field for research work at high temperatures we are not confronted by the fact of a physical limit existing; which may be approached but never reached. We can imagine no limit to possible increase of temperature, such as is the absolute zero a limit of decrease. While we may actually employ in electric furnaces temperatures, which, according to Moissan, have a lower limit of 3500° C., we can realize the possibility of temperatures existing in the stars measured by tens of thousands or hundreds of thousands of degrees of our temperature scale.

The moderate increase of working temperature given by the

electric furnace, enabled Moissan and others to reap a rich harvest of experimental results, and the natural inference is that much more might be expected from further extensions of the limits. These limits are, however, already set for us by the vaporization of all known substances. Our furnace itself keeps down the temperature by melting and volatilizing. We may indefinitely increase the energy in an electric arc and thus add to the heat evolved, but the addition only goes to vaporize more material. The limit of work then seems to be readily reached in the electric furnace, no materials for lining being available, not subject either to fusion or vaporization, thus using up the energy which would otherwise go to increase the temperature.

A suggestion as to a possible extension of temperature range may be made here. It may be requisite to work with closed receptacles under pressure, and to discharge through them electric currents of so great energy-value as to attain almost instantaneously the highest temperatures, to be maintained for only a very short time. We may imagine a huge condenser charged to a potential of say 10,000 volts as discharged through a limited body of gas contained in a small space within a strong steel tube which has a lining of refractory non-conductor. The energy may thus possibly be delivered so suddenly to a very limited body of material as to result in a momentary elevation of temperature passing all present known limits and capable of effecting profound changes in molecular constitution. We need all possible extension of the limits of research in this direction in order to discover some clue to the relations which the chemical elements bear to each other. The limit of possible strength of the containing receptacle, ors ome unforeseen factor, would probably set the new bounds. The point to be here enforced, however, is that far beyond any increase of working range in temperature, obtained in any way, there must still exist a further range unattainable by our best efforts and possibly forever outside of the field of experimental research. Our knowledge of this higher range can alone be derived from a study of the actions going on in the stars and nebulæ.

As with the temperature range so it is with the pressure

range. We may easily work under conditions which involve no pressure, but when we attempt to conduct our inquiries with increase of pressure, we soon find a limit to the tenacity of our strongest vessels, or to our ability to produce and maintain extreme pressures. We may work, not easily it is true, with pressures up to a few tons to the square inch, but this is as nothing compared with the condition which we know must exist within the larger celestial bodies; without reference to their condition, solid, liquid, or gaseous. Can we ever hope to experimentally reproduce the condition of a mass of gas so compressed that in spite of a very high temperature its volume is less than that of the same mass cooled to solidification? Yet this extreme of condition must be the normal state within the bodies of many of the stars.

It has been aptly said that many, and perhaps most, of the important discoveries have been made with comparatively simple and crude apparatus. While this may be true, yet it is probably true also that future advance work is likely to require more and more refined means and greater nicety of construction and adjustment of apparatus. The expense or cost, if not the difficulty of the work, may become so great as to effectually bar further progress in some fields. When instruments require to be adjusted or constructed, to such refined limits as a fraction of a wave-length of light, but few can be found to undertake the work. The interferometer and echelon spectroscope of Michelson involve such minute adjustments, that a wave-length of light is relatively thereto a large measure. It is well known that this comparative coarseness of light waves imposes a limit to the powers of optical instruments, as the microscope and telescope, such that no perfection of proportion, construction and correction of the lenses can remove.

In most fields of research, however, progress in the future will depend in an increasing degree upon the possession by the investigator of an appreciation of small details and magnitudes, together with a refined skill in manipulation or construction of apparatus. He must be ready to guide the trained mechanic and be able himself to administer those finishing touches, which often mark the difference between success and

failure. There must be in his mental equipment that clear comprehension of the proper adjustment of means to ends, which is of such great value in work in new fields. He must also learn to render available to science the resources of the larger workshops and industrial establishments.

The application of physical principles upon a large scale in such works, has frequently, in recent years, resulted in great gains to science itself. The resources of the physical laboratory are often relatively small and meagre compared with those of the factory. Experimental work in certain lines is now frequently carried on upon a scale so great and under such varied conditions as would be almost impossible outside of a large works.

In no field has this been more true than in that of electricity during the past few years. We need only instance the progress in alternating currents and in relation to the magnetic properties of iron. In large scale operations, effects which would be missed or remain masked in work undertaken upon a more restricted scale, receive emphasis sufficient to cause them to command attention. The obstacle of increasing costliness of equipment, which in some fields might act as a bar to further progress, can only be overcome by more liberal endowments of laboratories engaged in advance work. Even those in the community who can only understand the value of scientific work when it has been put to practical use, may find in the history of past progress, that many discoveries in pure science, which had not, when made, any apparent commercial importance or value, have in the end resulted in great practical revolutions.

Could Volta, when he discovered the pile one hundred years ago, have had any idea of its importance in practical work? or, did Davy or his contemporaries at the time of his experiments with the arc of flame between the charcoal terminals of his large battery, have any suspicion that in less than one hundred years, the electric arc would grow to such importance that more than 100,000 arc lamps would become a single year's production in this country alone. Faraday, when he made his researches upon the induction of electric currents from magnetism, could not have had any idea of the enormous prac-

tical work in which the principles he dealt with as facts of
pure science, would find embodiment.   When he wound upon
the closed iron ring the two coils of wire which enabled him
to discover the facts of mutual induction, he had begun, with-
out any suspicion of the fact, the experimental work which
gave to science and to practice, the modern transformer, now
built of capacities ranging up to 2500 H.P. each, and for
potentials of 40,000 to 60,000 volts.

These examples, and many others which might be given,
should convince even the most arrogantly practical man of the
high value of scientific research, not alone as adding to the
sum total of knowledge and for the admirable training it gives,
but because it cannot fail to have an ultimate practical effect.
Discoveries which at first seem to have no useful nor practical
outcome are often the very ones which underlie development
of the greatest importance in the arts and industries.

The work of Hertz upon electric waves was to the physicist
a grand experimental demonstration, tending to prove the
truth of the electromagnetic theory of light, and subsequent
progress was profoundly influenced by it, though no practical
use followed at once.   The physicist to-day may see in the
wireless telegraph only an extension of Hertz's original work,
for he need not consider the commercial or economic outcome.
He may, however, recognize the fact that in the wireless tele-
graph as developed by Marconi, practice calls for a broader
theoretical view.   Certain elements of construction and adjust-
ment of apparatus at first used and regarded as essential from
a theoretical standpoint, have already been laid aside.   The
radiator with its large polished brass spheres and special spark
gap has been found of no more effect than the simple pair of
small balls ordinarily constituting the terminals for high poten-
tial discharges.   It has been found that the transmitting and
receiving apparatus do not require to be attuned and that the
receiving coherer is not the true recipient of the electric wave
or disturbance in the ether.

These later developments are in fact departures, more or
less wide, from the principles underlying the Hertz demonstra-
tion.   A vertical wire is charged to a high potential and dis-
charges to earth over a spark gap.   During the discharge the

wire becomes a radiator of electromagnetic pulses or waves, regardless of the spark radiation. The receiving vertical wire is likewise alone relied upon to absorb the energy. Being in the path of the electro-magnetic wave conveyed in the ether from the transmitting wire, it becomes the seat of electromotive forces, which break down the coherer. This, in substance, may be considered as a series of small or microscopic spark gaps which can be crossed by the comparatively low potentials developed in the receiving wire. We are thus taught to recognize the fact that the refinements in methods and apparatus needed for a delicate physical demonstration, as of the Hertz waves, in this instance, may often be laid aside in practical application, where the end to be achieved is different. The sudden discharge of the Marconi transmitting wire may possibly give rise to a series of oscillations or high frequency alternating waves in the wire, but since the first half of the first wave at each discharge will have the greatest amplitude, it is doubtful if those which follow in the short train have any decided effect upon the receiver. According to this view, the fact of the discharge being oscillatory may, indeed, have no essential relation to the work done, but may be an unavoidable incident of the very sudden discharge which itself would set up a single pulse in the ether sufficiently intense for the work even if unaccompanied by lower amplitude oscillations following the first discharge pulse.

Before leaving the consideration of this most fruitful field of experimental research opened by Hertz, it may be stated that the one gap in the work yet to be filled is the actual production of electric waves of a wave-length corresponding to those of the spectrum. If this could be done by some direct method, no matter how feeble the effect obtained, the experimental demonstrations of the electric nature of radiant heat and light would be fitly completed. Several years ago it occurred to me that it might be possible to devise a method for accomplishing the end in view, and so close the existing gap. Many years ago an observation on sound echoes showed clearly the production of high pitch sounds from single pulses, or lower pitch waves. A bridge over a mile in length was boarded at the sides, and vertical slats regularly and closely

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placed along its side formed, for a sound wave incident thereon, a series of reflecting edges or narrow vertical surfaces, a kind of coarse grating. It was found that a loud sound or pulse, such as that of a gun shot, emanating from a point near one end of the bridge and two to three hundred feet in a line from the structure, was followed by an echo which was in reality a high pitch musical tone. The pitch of this tone corresponded to the spacing of the slats in the bridge considered as a reflecting grating for sound.

Following this principle it seems possible that a very sudden pulse in the ether or electromagnetic wave, incident at an angle upon a reflecting grating having from 20,000 to 40,000 ruled lines to the inch, if the plane of incidence were at right angles with the rulings might be thrown into ripples of the wave length of light and yield a feeble luminosity. If the color then varied with the angle of incidence chosen and with the angle through which the reflection passed to the eye, the experiment would be conclusive.

Despite the diligent studies which had been made in the invisible rays of the spectrum, both the ultrared and ultraviolet, a work far from completion as yet, the peculiar invisible radiation of the Crookes tube remained unknown until the work of Lenard and Roentgen brought it to the knowledge of the world. The cathode discharge, studied so effectively by Hittorf and Crookes, and by the latter called " radiant matter," was but a part of the whole truth in relation to the radiation in high vacua. It is needless to recount the steps in the discovery of Roentgen rays. We now know that these rays come from the impingement of the " radiant matter " or cathode rays. We know also that the higher the vacuum and therefore the higher the electric potential needed to effect the discharge, the more penetrating or the less easily absorbed is the resulting radiation. Rays have been produced which in part pass through cast iron nearly an inch thick. The iron acting as a filter absorbs all rays of less penetrating power. A question may here be put, which it will be for future experiment to answer : Can we by increasing the degree of vacuum in a Crookes' tube by the employment of enormous potentials for forcing a discharge through the higher vacuum, produce rays of

greater and greater penetrating power? What, in fact, may be the limit, or is there any limit to the diminution of wave-length in the ether, assuming for the moment that this invisible radiation is somewhat of the same nature as light, but of higher pitch, though it may be unlike light in not representing regular wave trains.

Roentgen radiation while spoken of as invisible, is in reality easily visible if of great intensity. The parts of the retina which respond and so give the sensation of luminosity are apparently those around the eye and not directly opposite to the iris opening. These parts of the retina sensitive to the rays are characterized by the preponderance of "rods," giving the simple sensation of illumination, apparently white in the case in question. The "cones," or those portions of the retinal membrane whose function is believed to be the recognition of color or differences of wave-length, appear not to be excited by the Roentgen radiation, or only very feebly. If this be true it would account for the less intensity of the luminous effect upon those portions of the retina near the optic axis of the eye. All this favors the view that the Roentgen radiation is without sustained pitch or wave trains, and resembles more a sharp noise or crash in sound.

For pressing experimental work in the highest vacua to its limit, as above suggested, we already have means at command for the production of the most complete exhaustions, requiring extremely high potentials to pass an electric discharge. We have also in well-known forms of high frequency apparatus the means for producing electromotive forces limited only by our means for insulation. A recent apparatus devised by me and called a dynamostatic machine, gives equal capability of producing high potentials of definite polarity, positive and negative. It should not be long, therefore, before work is undertaken in this suggested direction of pressing this matter of rays of high penetrating power much farther than has been done. The question arises whether any such rays can exist which are not appreciably absorbed in passing through dense substances. They would probably not affect a photographic plate nor a fluorescent screen. If they lost also the property of ionizing a gas and causing electric convection, we might

not even be able to discover them. That some influence or action in the ether does actually penetrate the dense masses in space is evidenced by gravitation, the mystery of mysteries. We are, however, not justified in going beyond the proved facts which can only be the result of experimental work and close observation. All else is speculation. The energy source of the Becquerel rays is another mystery apparently far from being cleared up, and if it be true, as recently announced, that a substance named radium, has in reality nine hundred times the power of emitting these rays than is possessed by uranium and thorium, and that the radiation is able to cause visible fluorescence of barium platinocyanide, the mystery but deepens and makes us again think of the possible existence of obscure rays only absorbed and converted by a few special substances.

The diffusion which takes place when Roentgen rays pass through various media is another phenomenon which needs more attention from investigators. This effect seems to be produced by all substances in a greater or less degree. It, however, appears to be nearly absent in the case of those substances which give out light or fluoresce under the rays, as barium platinocyanide and calcium tungstate. It will be important to determine definitely whether the rays diffused by different substances are lowered in pitch or penetrating power as compared with the rays exciting the diffusion ; whether, in other words, the rays from a tube with quite high vacuum, excite similar rays by diffusion, or rays more absorbable ; and if a lowering takes place, whether it occurs in like manner and degree for all diffusing media.

The phenomenon may be akin to fluorescence, as when quinia sulphate converts the invisible ultraviolet rays of the spectrum into lower rays or visible light. The action may be at its extreme when barium platinocyanide excited by Roentgen rays, so lowers the pitch as to produce rays within the visible spectrum, for this compound gives very little or no Roentgen ray diffusion. Are there substances which under Roentgen rays fluoresce with invisible rays of the order of the ultraviolet of the spectrum? If, as is the case with solid paraffine, the irradiated substance give rise to considerable diffusion, it can, as I have noted, produce a secondary diffu-

sion in other masses of the same substance, or of other substances, as indicated by feeble fluorescence of the sensitive barium salt, thoroughly screened from the direct source of rays and from the first or primary diffusion. It is probable that tertiary diffusion could be found if we possessed a far more powerful or continuous source of the rays for exciting the initial diffusion. The ray emission, even in the most powerfully excited tube is probably so intermittent that the active period is but a fraction of the total time. It may easily be that the limit of intensity of Roentgen ray emission has not yet been reached, especially when artificially cooled anticathode plates are available.

There is much room for experimental work in this fascinating field. We need for it the means for the production either of a continuous electric discharge at from 60,000 to 100,000 volts, or a high frequency apparatus capable of giving an unbroken wave train, that is, a succession of high period waves of current without breaks or intermissions.

The ordinary high frequency apparatus for obtaining discharges of high potential from alternating currents, gives only a rapid succession of discharges each consisting of a few rapidly dampened oscillations. These discharges occupy but a small fraction of the total time. This is very different from a continuous sustained wave train, with the successive waves of equal amplitude following each other without break. Such sustained waves would doubtless be of use in research, especially in vacuum tube work, and they would of course convey much more energy than the usual broken or interrupted discharge known as a high frequency discharge.

Some six or seven years ago I endeavored, while working upon the subject of high frequency, to fill the gap. The result was an apparatus which, with its modifications, deserves more study and experiment than I have been able to give to it. A brief description may not be out of place. A large inductance coil with a heavy iron wire bundle for a core, a coil of relatively few turns with no iron core, and a condenser of variable capacity, were connected in series across the mains of a 500-volt electric circuit. The smaller coreless coil and the condenser were arranged to be shunted by an adjustable spark

gap with polished ball terminals.   By simply closing for a moment the spark gap so as to form a low resistance shunt around the condenser and the small coil, and afterward slowly separating the balls, the local circuit of the condenser, small coreless coil and shunting gap become the seat of sustained oscillations, the frequency of which depends upon the relation of inductance and capacity in the local circuit.   The energy supplied is that of a continuous current through the large inductance coil with the heavy core.   The action of the apparatus is easily comprehended by a little study.   The oscillating current in the local circuit may be made to induce much higher potentials in a secondary circuit inductively related thereto.   In this case the turns of the secondary in relation to the primary, are, as usual, such as to step-up the potential.   In other words the potential developed in the secondary is determined by the transforming ratio.

We thus have a high frequency apparatus in which the waves are sustained in an unbroken series, and we employ as the source of energy a continuous current circuit.   It shows that we may continuously supply energy to an oscillating system and so keep up the amplitude of electric oscillations, the frequency of which is that due to the capacity and inductance of the part of the circuit in which oscillations are set up.

While, in the forms of high frequency apparatus alluded to, we may obtain almost any differences of electric potential up to millions of volts, assuming the apparatus large enough for the work, we do not get a sustained separation of positive and negative charges, as in the static machine, or in a less complete degree with the inductive coil.   Prof. Trowbridge, of Harvard, has, however, made use of large Planté rheostatic machines, the condenser plates of which are charged in parallel from 10,000 small storage cells connected in series.   The discharge of the condenser plates is effected after they are connected in series by a suitable connection changing frame moved for the purpose.   Very high potential discharges are thus obtained and the polarity is always definite.   It is manifest that the size of the apparatus and the perfection of its insulation determine the possible performance.   The objection to such an apparatus for experimental research or demonstration is the

large number of cells required and the complicated arrangements of circuits for charging them. I have, however, recently succeeded in removing all necessity for the presence of charging cells, and have produced what may be termed a dynamostatic machine which is worked by power or by current from a lighting circuit, either continuous or alternating, and may replace a static machine. It is, of course, not dependent upon the weather. I trust it may be of sufficient interest to merit the following brief description : A small electric motor has in addition to its commutator a pair of rings connected to its armature winding for obtaining alternating currents. The shaft of the motor drives synchronously a revolving frame bearing connections which, as in the Planté rheostatic machine, connect a series of condenser plates alternately in parallel for charging and in series for discharging at high potential. A small oil immersed step-up transformer has its primary connected to the brushes bearing upon the two alternating current rings of the motor, and its secondary, giving say 20,000 volts, is periodically connected to the condenser plates while in parallel, by means of the revolving connection frame. The adjustment is such that only the tops of the alternating waves or their maxima are used to charge the condenser plates, while, also, those halves of the waves which are of the same polarity are alone used, the others being discarded or left on open circuit. The apparatus may be driven by power, in which case the electric motor becomes a dynamo, exciting its own field and supplying alternating current to the primary of the step-up transformer, or, suitable alternating currents may drive it as a synchronous motor. Such a machine run by continuous currents and having only eleven plates, gives sparks between its terminals over twelve inches long in rapid succession. It can be built cheaply, and is a highly instructive machine from the transformations it illustrates.

The machine is also arranged, by the addition of a simple attachment, so that it may be used to charge insulated bodies, or to charge Leyden-jar condensers or the like, replacing the ordinary static machines. It might, in fact, be used to charge a second range of condenser plates in another rheostatic machine to a potential of 100,000 volts, for example. These,

after coupling in series or cascade, might be made to yield potentials beyond any thus far obtained.

The interest in such experimental apparatus and the results obtained, comes largely from the apparent ability to secure a representation of the effects of lightning discharges upon a moderate scale, and the possibility of studying the action of air and other gases, as well as liquids and solids, at varying temperatures and pressures under high electric stresses. Broadly considered, however, the similarity of the effects to those produced in a thunder-cloud is more apparent than real. The globules of water constituting the electrified cloud do not possess charges of millions of volts potential, the effects of which are seen in the stroke of lightning. The individual globules may possess only a moderate charge. When, however, they are massed together in a large extent of cloud the virtual potential of the cloud as a whole, with respect to the earth, may be enormous, though no part of the cloud possesses it. The cloud mass not being a conductor, its charge cannot reside upon its outer surface or upon its lower surface nearest the earth, as with a large insulated conductor. The charge, in fact, exists throughout the mass, each globule of water suspended in the air having its small effect upon the total result.

When the cloud discharges, the main spark branches within and through the cloud mass in many directions. The discharge can at best be only a very partial one from the nature of the case. These are conditions which are certainly not represented in our experimental production of high potential phenomenon, except perhaps upon a very small scale in the electrified steam from Armstrong's hydroelectric machine, a type of apparatus now almost obsolete. Yet if we wish to reproduce as nearly as possible upon a small scale, the conditions of the thunder-cloud, we will be compelled to again resort to it. In volcanic eruptions similar actions doubtless occur and give rise to the thunder-clouds which often surround the gases sent out from the crater.

Considering then that the conditions in the thunder-cloud are so different from those in our experiments with high potentials, we can easily understand that the study of lightning phenomena may present problems difficult to solve. Two

forms at least of lightning discharge are quite unknown in the laboratory; namely, globular lightning and bead lightning, the latter the more rare of the two. Personally I cannot doubt the existence of both of these rare forms of electric discharge, having received detailed accounts from eye witnesses. On one occasion, while observing a thunder-storm I narrowly missed seeing the phenomenon of globular lightning, though a friend who was present looking in the opposite direction, saw it. The explosion, however, was heard and it consisted of a single detonation like the firing of a cannon. According to the testimony of an intelligent eye witness, who described the rare phenomenon of bead lightning within an hour after it had been seen, it is a very beautiful luminous appearance like a string of beads hung in a cloud, the beads being somewhat elliptical, and the ends of their axes in the line of the discharge being colored red and purple respectively. This peculiar appearance, not at any time dazzlingly bright, persisted for a few seconds while fading gradually.

Again, our knowledge of the aurora is not as yet much more definite or precise than it is in regard to the obscure forms of lightning alluded to above. Whether these phenomena will ever be brought within the field of research by experimental methods is an open question.

The endeavor in the foregoing rather disconnected statements has been to indicate directions in which the field of experiment may be extended, and to emphasize the fact that research must be carried on by extension of limits, necessitating more liberal endowment of research laboratories. I have tried to make it clear that the physicist must avail himself of the powers and energies set in play in the larger industrial enterprises, and finally that the field of possible exploration in physics by experimental methods has its natural boundaries, outside of which our advances in knowledge must be derived from a study of celestial bodies.

The riddle of gravitation is yet to be solved. This all permeating force must be connected with other forces and with other properties of matter. It will be a delicate task, indeed, for the total attraction between very large masses closely adjacent, aside from the earth's attraction, is very small.

Scientific facts are of little value in themselves. Their significance is their bearing upon other facts, enabling us to generalize and so to discover principles, just as the accurate measurement of the position of a star may be without value in itself, but in relation to other similar measurements of other stars, may become the means of discovering their proper motions. We refine our instruments, we render more trustworthy our means of observation, we extend our range of experimental inquiry, and thus lay the foundation for the future work, with the full knowledge that although our researches cannot extend beyond certain limits, the field itself is, even within those limits, inexhaustible.

REPORTS READ.

REPORT OF THE COMMITTEE ON STANDARDS OF MEASUREMENT.   Read
by T. C. MENDENHALL, Worcester, Mass.

The last report of this committee related to the redetermination of the
electrochemical equivalent of silver by Drs. Patterson and Guthe by
means of a specially constructed electrodynamometer.   The result was
0.0011192 gm. per ampere per second.   During the past year the same
electrodynamometer has been employed in making an absolute determi-
nation of the electromotive force of the Clark standard cell.

Two cells were made by the secretary of this committee in accordance
with the specification which has been legalized in the United States.   The
two were made at an interval of a year, but at the same temperature they
do not differ more than one part in ten thousand.

The method adopted in measuring their E.M.F. was to compare the
E.M.F. of the two in series with the fall of potential over the resistance
of a coil of manganin wire immersed in paraffin oil, when the current
through it balanced the torque of the phosphor-bronze wire of the elec-
trodynamometer when twisted through one complete turn.   The compar-
ison was made by applying the Poggendorff method to the cells and to the
potential difference between the terminals of the manganin coil in succes-
sion.   Since the resistance of the manganin coil is involved in the meas-
urement, it must be known with accuracy ; for this purpose two standard
one-ohm coils were ordered from Wolff of Berlin with certificates from
the Reichsanstalt.   These coils arrived in June.

Three independent comparisons were made with the cells at 15° C.
The temperature was measured by immersing the cells in a bath of petro-
leum and taking the temperature by means of a thermometer made by
Haak of Jena, reading to fifths and calibrated at the Reichsanstalt.   Its
number is 11,021 end at 15° it reads 0.05° too low.

The result of the three measurements is 1.4333 volts at 15°.   Glazebrook
and Skinner[1] by the silver voltameter method and assuming 0.001118 as
the electrochemical equivalent of silver, obtained 1.4342.   Kahle found
by means of a Helmholtz electrodynamometer 1.4338.[2]   His value of the
silver equivalent is 0.0011182.   But Kahle says that silver nitrate neu-
tralized by silver oxide gives a larger value for the electrochemical equiv-
alent by five parts in ten thousand.   Neither Kahle nor Glazebrook ap-
pears to have employed the neutralized silver nitrate solution.   The value
0.0011192 was obtained by the use of the neutralized solution.   Hence the
silver equivalent for the solution used by Glazebrook equals 0.0011187.
Assuming this value instead of 0.001118, Glazebrook's result is reduced
from 1.4342 to 1.4332.   This is practically identical with the value now
reported by the secretary of this committee and Dr. Guthe, which is
1.4333.

[1] *Phil. Trans.*, (A), Vol. 183, p. 567, (1892).
[2] *Zeitsch. f. Instrum. Kunde.* June, 1898.

PAPERS READ.

An Apparatus for the Demonstration of the Varying Currents in the Different Conductors of a Rotary Converter. By F. C. Caldwell, Ohio State University, Columbus, Ohio.

This apparatus consists of (*a*) a disk of paper board, having a narrow slit cut along a radius, and occupying nearly the whole of the latter, on the edge of which slit is a scale of plus and minus amperes, with the zero at the middle ; (*b*) a paper disk, placed under disk (*a*) and marked with a datum circle under the zero of current, and a sine curve drawn with radial ordinates, its maximum being equal to the maximum alternating current supplied to the converter. Also two lines parallel to the sine curve, and radially distant therefrom by the amount of the direct current; (*c*) a back of paper-board, upon which are represented two pole-pieces and two brushes, and upon a pivot in the center of which disks (*a*) and (*b*) turn ; (*d*) strips representing collector ring connections, and which can be attached on disk (*a*) either as one, three, or four phase, and at any angle relatively to the slit. The collector ring connections being so placed, and the disk (*b*) being so attached to the back that the maximum of the sine-curve will come under the slit when the section of the armature [disk (*a*)] including the slit is under the center of the pole-piece, by revolving disk (*a*) the ordinate cut off by the slit from one or the other of the lines parallel to the sine curve will give the instantaneous current in the conductor represented by the slit. The different current curves in the conductors differently situated with reference to the collector connections may be studied by changing the points of attachment of the latter relatively to the slit. Other phenomena may also be studied.

A New Graphical Method of Constructing the Entropy-Temperature Diagram from the Indicator Card of a Gas or Oil Engine. By Henry T. Eddy, Univ. of Minn., Minneapolis.

The equation of perfect gases

$$pv = ct$$

is assumed to apply with sufficient accuracy to the working fluid in the cylinder of any such engine; and by taking successive equidistant values of p the relations between the variables v and t regarded as coordinates

will be represented by successive sloping lines through the origin, one line for each assumed value of p. By the help of these lines a $v\,t$ or volume-temperature diagram may be constructed from any given $v\,p$ diagram such as an indicator card is. For, the sloping lines in $v\,t$ coordinate correspond to horizontal lines in $v\,p$ coordinates, and the corresponding points have the same values of v; i. e., are on the same vertical. Having thus found a sufficient number of points on the $v\,t$ diagram corresponding to the $v\,p$ card, its contour is readily obtained.

Again, from the equation for difference of entropy for perfect gases; viz.,

$$E = 2.45 \log T + \log V$$

in which E, T and V are proportional respectively to difference of entropy, to absolute temperature and to volume, it is evident that E can be constructed from the logarithms of the volume and temperature. This paper goes on to show how this may be done conveniently and accurately by graphical methods, and in so doing treats and discusses, as examples, typical cards of the Diesel oil engine and Otto gas engine.

In progressing by the process first mentioned from the $v\,p$ to the $v\,t$ diagram the abscissas v remain unchanged, while the ordinates t take the place of p : and in the second part of the construction in passing from the $v\,t$ to the $e\,t$ diagram the ordinates t remain unchanged, while the abscissas e take the place of v. Either of these diagrams show the relative values of some one pair of the four variables volume, pressure, temperature, and entropy, and so represent the cycle of the working substance in accordance with the original suggestion of Professor J. Willard Gibbs by whom the entropy-temperature diagram was first proposed.[1]

COMPOUND HARMONIC VIBRATION OF A STRING. Part of a research aided by the Hodgkins Funds of the Smithsonian Institution. BY WILLIAM HALLOCK, Columbia University, New York, N. Y.

This paper consists essentially of a set of curves, showing the successive positions of a string vibrating under the influence of a fundamental and the first seven overtones. Each curve shows the position of the string at a particular instant. Sixteen such curves are shown, for the first sixteen sixty-fourths of a complete period of the fundamental. The amplitude of the components is proportional to the wave-lengths, in each case.

Thirty-one points were computed for each curve.

Each curve is computed from the formula :

$$y_1 = \left(a \sin 2\pi \frac{t_1}{T_1} \right) \sin 2\pi \frac{x_1}{\lambda_1} + \left(b \sin 2\pi \frac{t_1}{T_2} \right) \sin 2\pi \frac{x_1}{\lambda_2} + \text{etc.}$$

$$+ \text{ etc. } \div \left(h \sin 2\pi \frac{t_1}{T_8} \right) \sin 2\pi \frac{x_1}{\lambda_8} \quad (1)$$

1 Trans. Conn. Acad. Sci., Vol. II, part 2, April, 1873.

$$a = 2b = 3c = 4d = 5e = 6f = 7g = 8h \qquad (2)$$
$$T_1 = 2T_2 = 3T_3 = 4T_4 = 5T_5 = 6T_6 = 7T_7 = 8T_8 \qquad (3)$$
$$\lambda_1 = 2\lambda_2 = 3\lambda_3 = 4\lambda_4 = 5\lambda_5 = 6\lambda_6 = 7\lambda_7 = 8\lambda_8 \qquad (4)$$

A NEW FORM OF ELECTRICAL CONDENSER HAVING A CAPACITY CAPABLE OF CONTINUOUS ADJUSTMENT. BY LYMAN J. BRIGGS, Physicist, U. S. Dept. of Agr., Washington, D. C.

This condenser consists of a stack of spring brass and mica sheets, arranged in the usual manner. The spring brass plates are given a curved form, so as to present an open structure when the condenser is assembled. The capacity of the condenser can consequently be continuously varied by compressing the system of plates. The condenser can be used either in air or oil.

THE TIME OF PERCEPTION AS A MEASURE OF THE INTENSITY OF LIGHT. BY J. MCKEEN CATTELL, Columbia University, N. Y. City.

Photometric methods must depend on comparison of usual sensations. It is easy to say when two lights are about equal, but the sensations have not been measured, the Weber-Fechner attempt having failed. The method used by the writer is to measure the time it takes to perceive a difference in the intensity of two lights. The greater the difference the less is the time of perception, and the times are equal when differences are equally great for consciousness. It is thus possible to find a gray usually midway between black and white, and to measure equal increments in sensation. Disparate sensations can thus be compared and color-blindness and other defects of sensation can be discovered.

RELATIONS OF TIME AND SPACE IN VISION. BY J. MCKEEN CATTELL, Columbia University, N. Y. City.

If the eye is moved so that the line of vision sweeps over black and white bars with a hundred or even a thousand stimulations per second there is no fusion. If one light and then another affect the same retinal area they may be seen side by side, each observer seeing them in a way peculiar to himself. In ordinary vision a series of changes in time of the images on the retina is given in perception as a series of objects side by side. These experiments indicate that the phenomena of vision are not chiefly retinal, but cerebral, and that our perceptions are not 'copies' of an external world, but mental changes dependent on experience and utility.

THE MUSICAL SCALES OF THE ARABS. BY CHARLES K. WEAD, U. S. Patent Office, Washington, D. C.

The views of the Arab scales quoted by Helmholtz and others from Kiesewetter and Villstean must be recognized as unsound, now that Dr. Land has given a translation of the " Book of Music" of Al Farabi, who died 950 A. D. This proves that musicians demanded different scales for different instruments, although those for the short-necked lute were the most important. This lute had five strings tuned in fourths ; for the little finger of the left hand a ligature (fret) was tied around the neck at a quarter-length of the string, so giving a fourth; in the space up to the nut eight other ligatures were tied according to various rules ; some by calculating intervals of a tone as the Greeks did, others by bisecting the linear distance between two ligatures: thus arose a scale of twenty-two steps to the octave. But the notes due to the bisections fall in close pairs: so two steps in each tetracord, or five in the octave, were very short; accordingly later theorists substituted for each pair a third note determined by Greek principles and thus obtained the 17-step scale that has provoked so much controversy. Out of these many-note scales, various short scales or " modes" were selected for musical performance; these modes usually called into exercise only three out of the four fingers, and so had eight notes to the octave.

On the long-necked tambours the ligatures were located by almost entirely different principles, the most important being a " step-by-step tuning," nowhere else described in musical history ; as applied on the two-stringed tambour of Bagdad, this principle gave within a total compass of about a minor third a scale of eight notes.

MEDIEVAL ORGAN PIPES AND THEIR BEARING ON THE HISTORY OF THE SCALE. BY CHARLES K. WEAD, U. S. Patent Office, Washington, D. C.

The question of the origin and basis of the scale currently accepted by European musicians is now receiving much attention. Some vigorous writers maintain that it is based wholly on harmonic considerations, which apply also with equal force to all peoples who have not been led astray by instruments. I feel most decidedly that this conclusion is a hasty generalization, which has taken account only of a few classes of facts, and especially has ignored the history of the development of the European scale. Both as a contribution to one branch of this history, and as a matter having independent interest, a brief account is to be given of the history of organ-pipe tuning from the earliest times of which we have knowledge of it ; no English writer refers to the matter at all, and the few Germans who quote some of the documents of importance here do not consider their bearing on the subject of the scale.

Not much is known of the organs prior to the tenth century. The broad idea of blowing a series of whistles (fistulae) by air compressed by a bellows belongs, as is familiarly known, to classical times, and in our modern histories of the instrument there are various references to organs during the Dark Ages, as one sent to Pepin (757 A. D.), and one to Charlemagne a little later, which last is said to have been the first organ used in a church in the West; but by the close of the eleventh century instruments were in use in several places. Rimbault quotes a bungling, sometimes unintelligible, translation of an XIth century MS., which gives many details of the manufacture of pipes. But a far more important as well as earlier account is found in a MS. of the Xth century, from the German translation of which in Wangemann's Geschichte der Orgel we quote at length. By way of perface it should be noticed that all musical theory underlying these mechanical rules is due to Boethins (c. 525 A. D.), whose somewhat ignorant Latin compilation from Greek musical writers served men admirably during the years when scholasticism demanded, not experimental truth, but authority. So the Greek ideas of three genera, of tetrachords, of ditones instead of our major thirds, of Pythagorean ratios, of divisions of the monochord, etc., are everywhere to be found in these medieval writers. At the same time there is a good deal of variety in their practical ways of teaching the subject, and we shall see how they gradually emancipated themselves from these fettering theories.

This Xth century MS. gives many practical details about the manufacture of organ pipes. They were to be made of thin sheet copper rolled into cylinders about four feet long, all having the diameter of a pigeon's egg (a little over an inch). The lengths were measured from the mouth up. "And now since it is the diatonic genus in which at present for the most part songs move, the pipes are measured as follows: The first, which is smaller and therefore higher than all the rest, must be divided into eight parts, and by an eighth part of the first must the second be greater than the first, in order that they may differ by a tone. Just so the third must be greater than the second by an eighth part, and a tone lie between them. Then it must be so arranged that the fourth is greater than the first by the third part of the first, so that it differs from the first by a fourth, and from the third by a half-one. And the fifth must be greater than the first by a half of the first, so that it forms the pure fifth to it, but a tone with the fourth. The sixth must be greater than the fifth by an eighth of the fifth, and have a tone between them. The seventh must be greater than the fourth by a third part of the fourth in order to form with it a fourth, but a half-tone to the sixth. The eighth has the double length of the first, and is distant from it by a pure octave, which is always made up of a fourth and a fifth. The same operation as in the measurement for the second pipe is to be repeated to determine the series from the octave up in the order that we have given. With the seven tones of the octave described one can by rising and falling produce every song." Then follow details of mechanism—several pipes in unison or octaves, sometimes as many as five or ten, might be arranged to each

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valve, the longer pipes being at the player's right hand.· Connected to the valves by iron wires were certain wooden plates (keys?) bearing the "letters of the alphabet written twice, thus :

<div align="center">A B C D E F G A B C D E F G H</div>

in order that the player may more quickly see which plate he should strike."

A second rule for pipe lengths is then given, as if this anonymous MS. was a collection from various sources; it was thus: "He who would know the measures and construction of an organ must first of all imagine eight pipes having the same length and thickness, but all larger above than below (i. e., conical). Then take the first, which may be long or short, at pleasure; to find the relation of the second to it divide the first into nine parts and make the second equal to 8:9 of the first; similarly divided the second pipe into nine parts, and give the third again 8:9 of the second; to get the correct measure of the fourth pipe give it 3:4 of the first. The fourth pipe divide into nine parts, and the fifth must be in length 8:9 of it, as well as the sixth 8:9 of the fifth. The seventh again is 3:4 of the fourth, while the eighth is 8:9 of the seventh. When these eight are ready one goes, in the same way as from the first to the eighth, from the eighth to the fifteenth, the octave of the eighth, and from the fifteenth to the twenty-second, the octave of the fifteenth." To each valve there are to be arranged two longer pipes and a shorter one placed between them " that the three pipes may give a consonance, the so-called octave;" apparently the compass was as before only two octaves. Wangemann seems to overlook the fact that these two rules give totally different successions, for the first is a descending scale—increasing pipe lengths; the second an ascending scale. The first nominally gives a series of intervals approximately the same as from our a down to A, while the second gives approximately G to g, thus :

	G	A	B	C	D	E	F	G	a
First Rule {	$\overline{\cdot}$	2	$1\frac{6}{7}$	$\frac{27}{1\frac{5}{9}}$	$\frac{4}{3}$	$\frac{4}{3}$	$\frac{8\frac{1}{1}}{1\frac{2}{8}}$	$\frac{8}{9}$	1
	$\overline{}$	1	$\frac{8}{9}$	$\frac{17}{16}$	$\frac{3}{4}$	$\frac{2}{3}$	$\frac{9}{16}$	$\frac{9}{16}$	$\frac{4}{3}$
Second Rule	1	$\frac{8}{9}$	$\frac{8\frac{1}{1}}{1}$	$\frac{3}{4}$	$\frac{3}{4}$	$\frac{1\frac{8}{2}}{7}$	$\frac{9}{16}$	$\frac{8}{9}$	

If these two scales have any note in common they agree throughout, for each number in the last line is 8:9 of the number directly above it.

But any one who has the slightest knowledge of organ-pipe construction knows that all these intervals would in practice be found quite flat, the shorter pipes being relatively too long, and even the octaves sounding together in the way just described would be very unsatisfactory. So it is interesting to see how early the inadequacy of these rules was recognized.

A MS. of the Xth century attributed by Gerbert to Hucbald gives the following rule, which Wangemann, who quotes it along with those just given, strangely says, offers nothing new : "If the pipes are of equal diameter and the greater contains the less twice in its length and in addition its diameter they will mutually sound the consonance diapason [octave]. * * If the greater pipe contains the less a·whole time and a third part

of its length besides, and also a third part of the diameter of the hollow [i. e., of the internal diameter], they will sound a diatessaron [fourth]." Other ratios given are, for the double diapason four times the length of the shorter pipe plus three diameters; for the diapenute, or fifth, one and a half lengths and half the diameter ; for a tone, one length and an eighth ; and for a semitone, one length and a sixteenth. These rules give intervals, but not directly a scale in which the semitones are definitely located.

· But Hucbald has several other rules, one of which gives the succession nominally as from C to c, thus :

ı   {   {{   {   {   {{   {{{   {   ·

In one passage he says the first (highest) pipe should have a length eight times its diameter.

Odo in the same century gives clearly a different idea of getting out his ratios, though the results are the same, and he brings in both b and b flat. He says :   " In the measures of pipes there are the notes

C   D   E   F   G   a   ♮   c   .

"The length of low C is to be taken at pleasure ; this is divided into four parts and one part being subtracted leaves the pipe F." His further details may be condensed to a line thus :

G = { C ; D = { G ; a = { D ; E = { a ; ♮[=b] = { E ; b[=b♭] = { F.

"Further, the skilful musician observes that these measures are established by fourths and fifths," quite in the spirit of nineteenth century tuners, only he worked by measure, they by ear.

By far the fullest account of rules for pipe-lengths is given in the tractate De Musica by one of the brothers of St. Gall, written in Old High German in the same tenth century. The MS. Gerbert used was very imperfect, and Riemann has corrected his readings by the aid of the fine Leipzig Codex. Not the least important point is the frequent implication that the instruments were to guide the voice ; so rules are first given for the lyre and psaltery ; but it is said to be difficult to get the length of strings right, for if too long they are scarcely sonorous and the tone is poor, while if too short the higher tones are thin. But he who measures off organ pipes avoids these difficulties. " It is said that a pipe for the first letter [A] one ell in length from its lip up is too short, and one of two ells is too long ; but those between the two having a length of an ell and a half are suitable." The only figures I find for the ell of St. Gall (unfortunately of much later date) give it as almost exactly 24 English inches ; so this lowest pipe would have been about 36 inches long, and have given a note between d and f (on the bass staff) of our modern pianos. The uncertainty is because we do not know the diameter of the pipe which was to be "so wide as pleases you." The rules, which are so long that it might be tedious to quote them, give an ascending scale, with both minor seventh (called synemenon) and major, all corrected for influence of diameter nearly in the same way as stated by Hucbald ; thus

" take from the length of the first pipe the eighth part of its width and divide it from the point down to the lip * * * into nine parts of equal size ; give eight of these to the second pipe ; this is its length from the tip up."

The same Leipzig Codez contains a curious rule that makes the ratio for a tone 7 : 8 instead of 8 : 9. The author starts from A and ascends in pitch, so obtaining results that may be tabulated as follows :

His notation	A	.	B	.	C	D	.	E	.	F	.	G	a
Modern notation	A	.	B	C♯	D	.	E	F♯	G	♯	a		
Or..............	C	.	D	.	E	F	.	G	.	A	.	B	C
	1	.	⅞	.	??	?	.	??	.	???	.	????	?

Riemann treats this ratio 7 : 8 as the rough equivalent of Notker's ratio 8 : 9 together with this correction for diameter ; but this is inadmissible, for the fourth and octave are not corrected and so the two semitones are almost vanishingly small, or rather what should be the lower note, comes out the higher one !

Aribo, in the next century, gives Notker's rules, and others due to Monk Wilhelm with corrections based on different fractions of the diameter. And many more rules might be quoted.

Coming to more recent times, there is a little to be found in Father Kircher's voluminous Musurgia Universalis, published in 1650. He says the ratio of circumference to length of organ pipes varies very much, as from one-fourth to three-fifths; two-fifths was perhaps most usual (giving a diameter one-eighth of the length as stated by earlier writers). His lengths follow the familiar modern ratios, 1 : 2, 2 : 3, 3 : 4, 4 : 5, 8 : 9. He does not refer to any correction for diameter, which would be of large importance with such large pipes, nor does he speak of tuning the pipes after they are made. A century more shows a marked advance; for in the great book of Bedos de Celles, " L'art du Facteurs d'Orgues," Paris, 1766, while rules are given for fixing the pipe-lengths, proceeding by fourths and fifths, there are also directions for tuning by cutting the pipes off afterward to the exact desired pitch, and in the plates there are figures of tuning-cornets such as are used to-day. Lastly, in the great Encyclopédie (1750), under " Diapason," it was directed that to the computed length as given by such rules as the above some inches shall be added to allow for contingencies of tuning.

These citations are enough to show how slowly our ancestors, starting from the purely mechanical-mathematical scale inherited from the Greeks, and practically fitted only for a thin-stringed monochord, progressed to the series of notes of to-day, that is independent of any particular instrument. Historically, development of the scale has gone along with the development and perfection of instruments, first of the organ and then of the piano. In the organ the wind supply needed great improvements before a steady tone could be produced, and it was not till the invention of the wind-gauge in 1677 that this was fairly accomplished. Meantime other improvements had been going on ; the keys were narrowed, the

many pipes to a single key were distributed to registers, and pedals and black keys had been introduced. But all the time that the ideas of poly. phony and incipient harmony were growing, the king of instruments was not fitted to furnish a single interval that would be at all acceptable to-day. In fact Praetorius, who died in 1621, thirty years after Palestrina's death and seventy-five years after Luther's death, says one reason for the slow development of harmony was that "the tones and semitones were not turned correctly, and therefore the instruments or organs were not turned so 'justly' as at present." The errors of the old rule were very great; two pipes of 36 and 18 inches length above the lip and 2¼ inches diameter, according to Kircher's proportion, would not be an octave apart, but only a little over ten semitones, as C–A ; perhaps the early pipes were slimmer and the error less, so the corrected rule might give a fair approximation to correct intervals ; still the correction is not more than half or three-fifths of that required by the rules of the famous modern French organ builder, Cavaillé-Coll.

There is one more stage in the history of our scale. After the organ had been so far perfected that any desired intonation (e. g., just, mean-tone or equal temperament) could be given to it, keyed-stringed instruments were developed with not a little deliberate imitation of organ ideals, as those know who saw the Steinert collection at the Chicago Exposition. As the logical outcome of the demand on the part of the growing harmony for freer modulation into all keys there was a modification of the old Pythagorean and harmonic tunings, as well as of the mean-tone tempera- ment, finally resulting in the equal temperament This could be carried out conveniently on the stringed instruments ; it was more needed for the kind of music written for them, and the short duration of their sounds rendered the deviations of the tuning from perfect concords less offensive to the ear than when it was practiced on the long-drawn notes of the organ. But musicians found it unsatisfactory to try to maintain several standard scales, so the clavichord and piano have in spite of bitter opposition, forced their peculiar scale upon the European musical world, till orchestra, voices, and finally the organ have, with practical unanimity, surrendered to it. Of course this is a "survival of the fittest," but the statement only means the fittest for a particular environment ; for other environments it would not necessarily be the fittest ; e. g., that of an Oriental or savage musician, of a string quartette or of Europeans a century hence.

Finally it is to be observed that instruments have been the guides to the voice in all these ages under consideration. Guido, who died in 1050, taught his boys the intervals by the aid of the monochord, which he im- proved for this purpose. In later times the organs served a similar purpose, as appears from the remark of Praetorius, who says : "That the compass remained narrow for so long a time is because the organ was used only to accompany choral singing, and no great range was required, for harmony was unknown," and he distinctly says that only the bare choral in one part was performed on them. Even to-day what pupil learns to sing

intervals correctly except by directly or indirectly imitating an instrument? As instruments have developed, both the scales embodied in them and the ideas of musicians concerning the scale have changed, responding to distinctly traceable influences; and there is no hint in the long history that the "harmonic consciousness," on which to-day much stress is laid by some writers, has ever failed to content itself with the scale familiar to it, however wide the departures from a true harmonic scale. So if in the fields where harmony has won practically all its triumphs there is no proof of a scale-making "harmonic consciousness," may we not ask for substantial evidence that it exists among peoples who have no harmony? And may we not expect that ample explanation of the facts alleged in support of this view will be found when all the circumstances of the investigation are made known?

This brief presentation of one phase of musical history should convince the student that the opposing views regarding the basis of the scale so dogmatically presented by extreme physicists or extreme musicians are alike inadequate, because they disregard the historical elements of the problem.

NOTE.—The historical authorities for the principal statements made above are as follows : Rimbault, E. F.: The History of the Organ, London, 1870: Wangemann, O.: Geschichte der Orgel, Demmin, 1880, p. 66, 69, 70, 91. Gerbert, M.: Scriptores ecclesiastici de musica sacra, 1784, as reprinted in Migne's Patrologia Latina, Vols. 131, 132, 133, under the names of Hucbald, Odo, Notker, and Aribo. Reimann, H.: Studien zur Geschichte der Notenschrift, Liepzig, 1878, p. 298. The argument for the "harmonic consciousness" is strongly put by the late Professor J. C. Fillmore in Omaha Indian Music, Cambridge, 1893.

ELECTRICAL ANESTHESIA. BY E. W. SCRIPTURE, Yale University, New Haven Conn.

A method of producing anesthesia—or rather analgesia—by direct application of an electric current and without either internal or local application of drugs is based on the following principle : An alternating current—preferably a sinusoid—with equal positive and negative phases is made to traverse the nerve. At a proper frequency—about 5000 complete periods in a second—it can be made to cut off all sensory communication by this nerve. Needles can be run into the part of the body supplied by the nerve without any pain being felt. Groups of nerves ; e. g., the trachial plexus, can be cut off from communication in a similar manner. Experiments are now being made in the effort to get at the roots of the spinal nerves and thus cut off communication with the various parts of the body. Experiments have been tried in sending the current from the Gasserian ganglion along the superior maxillary nerve with a view to cutting off the teeth from communication, but the contractions of the

facial muscles have proved as yet too disturbing. The experiments have made evident the fact that with increasing frequency of alternation the muscular contractions decrease; it is proposed to raise the frequency still higher to avoid muscular contractions while retaining the analgesic effect on the nerves and a machine for this purpose is now being built.

AN ABSOLUTE DETERMINATION OF THE E. M. F. OF A CLARK CELL. BY H. S. CARHART AND K. E. GUTHE, Ph.D., University of Michigan, Ann Arbor, Mich.

The paper gives an account of the continuation of experiments made with an absolute electrodynamometer for the determination of electrical units. While last year the electrochemical equivalent of silver was redetermined, this year by practically the same method the E. M. F. of the Clark standard cell was found.

The result obtained was

$$E.M.F. = 1.4333 \text{ volts at } 15° \text{ C.}$$

QUANTITATIVE INVESTIGATION OF THE COHERER. BY AUGUSTUS TROWBRIDGE, PH.D., University of Michigan, Ann Arbor, Mich.

Since the very remarkable success of G. Marconi in the field of wireless telegraphy, a renewed interest has been felt by physicists in the coherer, the apparatus devised by Branley which has made wireless telegraphy possible.

Besides the original paper[1] of Branley there have been a number of communications on the subject of coherer action ; in particular those of Dorn[2] and Aschkinass,[3] which have thrown much light on the subject. As yet, however, the experimental data are not complete enough for a perfectly satisfactory theory of coherer action to be formed, those already existing being seemingly inadequate to explain all the observed phenomena.

Even if we do not know just what takes place in the coherer itself, we know that the cause of the fall in resistance is primarily an electromagnetic disturbance in the space surrounding the coherer. This disturbance will induce a static wave in a conductor whose direction in space coincides with the direction of the lines of electric force from the source of the electromagnetic disturbance. If this static wave is the cause of the lowering of the resistance of the coherer, then a static discharge from a Leyden jar, or electrophorus, through the coherer, should produce a like effect. This conclusion was verified by Professor Henry S. Carhart and

1 Compt. Rendus, cxi.
2 Wied. Ann., lxvi, p. 146.
3 Wied. Ann., lxvi, p. 284.

myself while we were engaged a few months ago in testing the sensibility of coherers for telegraphic purposes. If this conclusion is warranted, then on the nature of the discharge and the quantity of electricity which goes through the coherer should depend the fall of resistance of the latter.

The present paper has to deal with the lowering of the resistance as a function of the quantity of electricity discharged through and the difference of potential on opposite sides of the coherer.

In order to test the supposed relation between quantity of electricity discharged and the fall of resistance in the coherer, I adopted the following arrangement.

By means of a charge and discharge key, a subdivided condenser could be charged from a large storage battery, the electromotive force of which could be varied at will from 2 to 70 volts, and then discharged through the coherer. The capacity at my disposal could be varied from 3.2 microfarad to 0.05 microfarad, and this range, taken together with that of the storage battery, made it possible to vary the product $CV = Q$, within a comparatively large range, the upper limit of which, however, was only 224 microcoulombs.

For the purpose of measuring the fall of resistance, a branch circuit containing a single storage cell (E. M. F. $= 2$ volts) and a milliammeter was connected to the ends of the coherer. The limit of accuracy in reading the milliammeter was 0.001 ampere.

With a coherer of the type described by Marconi, I found, as I expected, an increase in conductivity with an increase in quantity of electricity sent through the coherer; however on increasing Q beyond a certain point, the corresponding increase in conductivity was very small; for the coherer in question, for example, the increase in conductivity was very rapid on increasing Q up to about 15 micro-coulombs, but beyond this point, doubling the quantity only produced a fractional change in the conductivity. It seemed as if the fifteen micro-coulombs were enough to produce all, or nearly all, the lowering possible for the coherer in question and for the given charging potential (a point I will explain presently).

As the results obtained with the above-mentioned coherer were not concordant enough for quantitative work, I adopted another type which I have since seen described in a paper by Branley,[1] in which he claims that it is more sensitive than the older form.

The coherer I used consisted of 21 bicycle pedal balls (diameter 3 mm.) mounted in a horizontal glass tube of about the same bore, the end balls being soldered to lead-wires and the tube being provided with a device for adjusting the pressure of contact between the balls. The normal resistance of this coherer was about 2000 ohms, care being taken always to return to about this resistance by tapping the tube and adjusting the pressure before an observation was taken on the fall of the resistance due to the discharge of the condenser. M. Branley used hard steel balls 12 mm. in diameter—those first used by me were 9.5 mm. in diameter, but I found the smaller ones finally used to be much better suited to my purpose, at

1 Comptes Rendus, No. 18, May, 1899.

least, and I should judge that this would also hold true for telegraphic purposes.

However, I made no attempt at long-distance telegraphy with my coherer, having only satisfied myself that, at short range, the ball coherer behaved in all respects like a filing coherer.

The method of observation was the following : With a given charging potential the capacity was varied through the limits above mentioned : for each capacity the mean of about 20 observations was taken on the lower limit of resistance attained by the coherer on discharging the capacity through it ; this resistance being indirectly obtained by Ohm's law from the readings of the milli-ammeter.

As I have mentioned above, the upper limit, or normal resistance of the coherer was arranged to have a uniform value of about 2000 ohms.

Next, the charging potential was given another value and a like set of observations taken, and so on through the range of the charging potential.

It was at once evident that the lower limits of the resistance was not the simple function (without reference to the factors of Q) of the quantity of electricity sent through the coherer that I had expected. If this had been the case, I should have found the lower limit of the resistance the same for the cases $Q = C_1V_1 = C_2V_2 = C_3V_3$, etc. = constant. From my observations this was distinctly *not* the case.

I found that for $Q = $ constant, the larger the charging potential—and hence the smaller the capacity—the lower was the final resistance or the greater the conductivity of the coherer after the discharge.

I have given the results of my observations on the ball coherer in curves 1 and 2. A table of the numerical values I obtained I have not given, since it would be of little theoretical import, as it would hold only for the coherer I used. The form of the curves given should, I believe, be similar for all coherers.

In curve 1, the abscissas represent the reciprocals of the lower limit of resistance of the coherer, or the conductivity; the ordinates, the quantity of electricity sent through. The seven different curves represent the change in conductivity as a function of the quantity for the seven different charging potentials used, the indices $V = 10, 12, 18$, etc., denoting these potentials.

As will be seen, all the curves tend in a general way, as they should, towards a point on the conductivity axis, very near the origin, a point which would denote the reciprocal of 2000 ohms, the normal resistance. Also it is clear that the greater the charging potential, the more rapid the rise of the conductivity per unit increase in quantity discharged.

I was unable, with the coherer used, to get coherer action when the charging potential of 8 volts and under was used. This point is brought out by the set of curves No. 2, in which the abscissas are the same as in curves No. 1, while the ordinates represent charging potentials. All the curves I = constant cut the ordinate axis at a point $V = 8.75$ volts. This is, so to speak, the critical voltage for the coherer under examination. Unless a potential difference between the terminals of the coherer of at

least this value is produced by the electromagnetic disturbance, no coherer action will take place.

Probably every coherer has a critical value of the potential difference peculiar to itself, a coherer with a low value being preferable for telegraphic purposes. Owing to lack of time I have not been able to experiment with more than one coherer.

PLATE I.
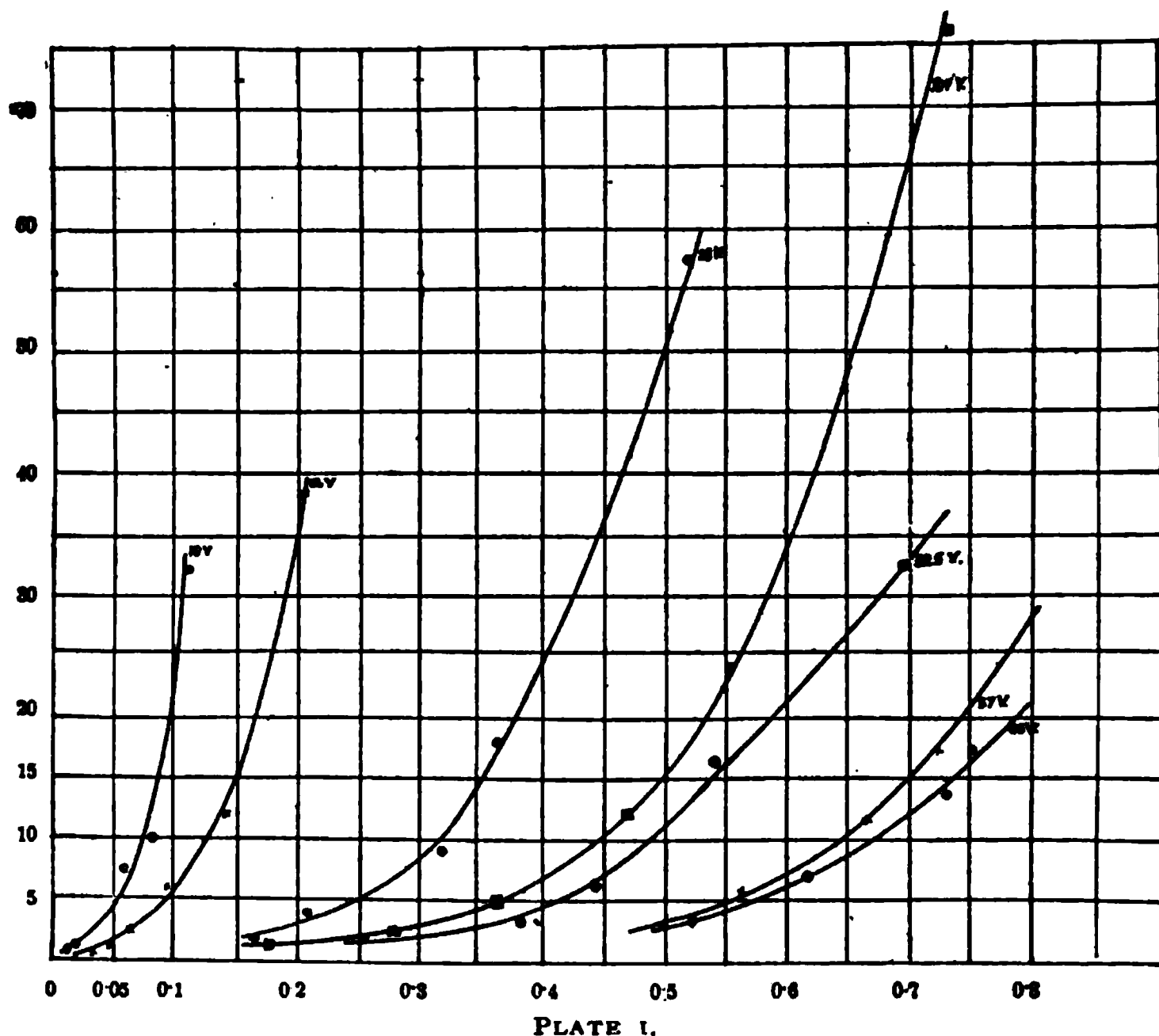
When we inquire into the nature of the discharge through the coherer which produces a lowering of resistance, the question arises as to whether or not it is of an oscillatory character.

From the theory of the oscillatory discharge of a condenser, we have a term

$$\sqrt{r_2 - 4\frac{L}{C}}$$

entering into the expression for the current strength I, where r is the resistance of the discharge circuit and L and C respectively the self-induction and capacity. If $r^2 < 4\frac{L}{C}$, an imaginary quantity enters into the expression for I, this is the condition for a periodic discharge. If, on the other hand, $r^2 > 4\frac{L}{C}$, we have an aperiodic discharge.

Under the conditions present in my investigation, the latter case was

certainly realized, for r normal was 2000 ohms, C from 3.2–0.05 microfarad and L certainly very small, say less than 0.01 quadrant.

$$r = 4.10^6 \text{ ohm} = 4.10^{24} - \text{cm. sec.}^{-1}$$
$$C = 0.05 \text{ microfarad} - 5.10^{-17} \text{ cm.}^{-1} \text{ sec.}^1$$
$$L = \text{say } 0.01 \text{ quadrant} = 10.10^6$$
$$4\,\frac{L}{C} = 4\ \frac{10.10^6}{5.10^{-17}} = 8.10^{23} < [4.10^{11} = r^4]$$

As L is certainly less than the value here assumed, the discharge circuit being a short straight copper wire, it is evident that $r^2 > 4\dfrac{L}{C}$, or that the discharge which affects the lowering of resistance in this investigation was aperiodic. I tested this point experimentally in the following manner : the condenser was removed and the lead-wires from the storage battery to the ends of the coherer were provided with a key and a thin
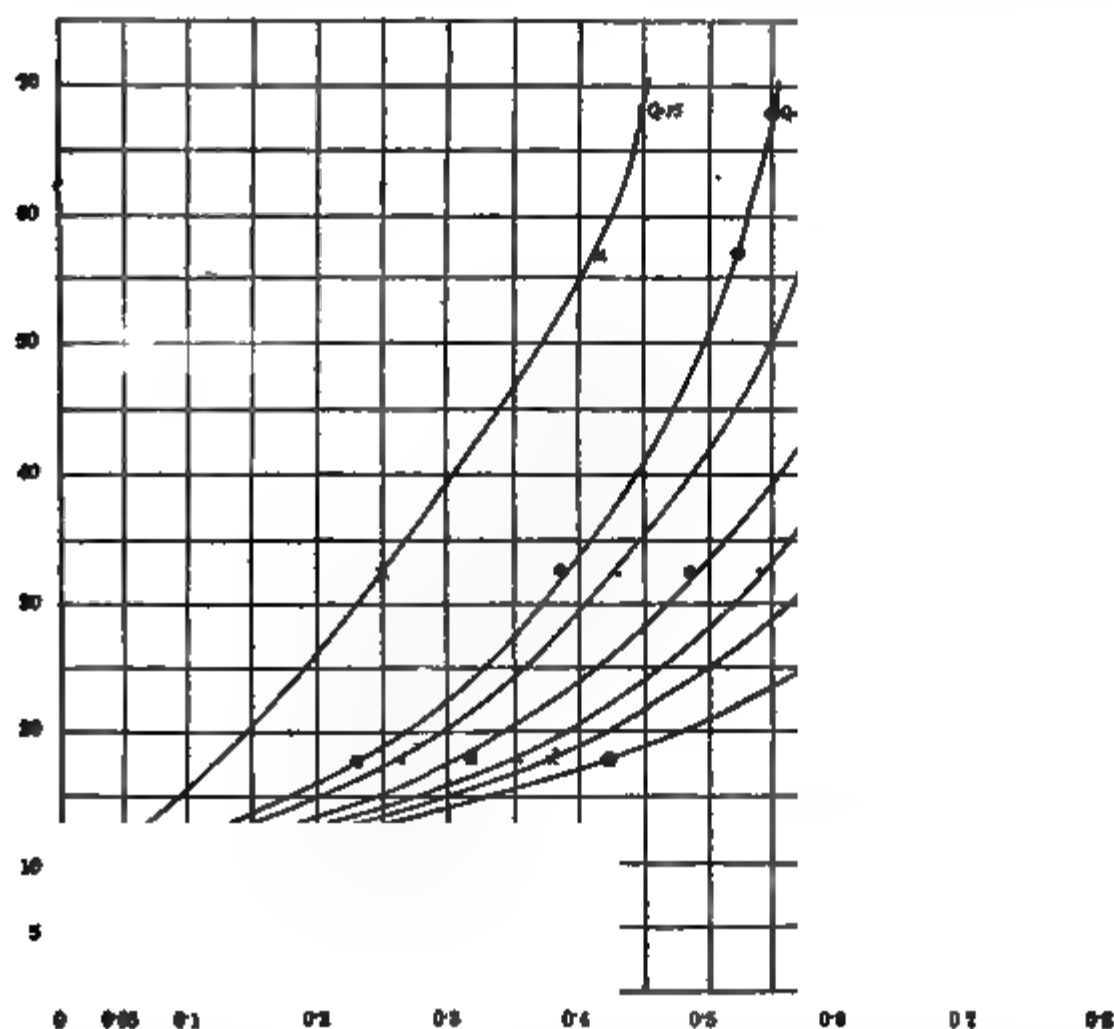
PLATE 2.

1/10 ampere fuse-wire in series. When the key was closed a steady current flowed through the coherer—the voltage of the storage battery was varied through a range of from 8–70 volts, and the lowering of the resistance of the coherer was again found to vary as the voltage. Owing to the element of time entering into the problem (the time elapsing from the closing of the key to the blowing out of the fuse), it was not possible

to make quantitative observations on the fall of resistance. It was evident, however, that a steady current did produce this drop in resistance, that the drop was greater the greater the electromotive force of the storage battery, and further, as I had before noted, that for an electromotive force less than from 8–10 volts no coherer action took place.

Now we can regard the coherer with this vertical wire and earth connection, as it is used by Marconi, as part of the discharged circuit of a condenser whose plates are the vertical wire and the earth. A certain potential difference between vertical wire and earth is caused by the electromagnetic waves sent out from the Herz oscillator; this potential and the capacity of the vertical wire earth condenser determine the quantity of electricity which is discharged through the coherer in the form of an aperiodic impulse and which produces the lowering in resistance necessary to wireless telegraphy. The potential difference on opposite sides of the coherer must be an inverse function of the distance between sending and receiving stations; if this distance is small, the potential difference being above what I have called the critical potential for the coherer used, if we increase the capacity of the vertical wire we shall increase the fall in resistance of the coherer, but if the distance is so great that the potential is at about its critical value, then an increase in capacity would lower the potential and so diminish or destroy the sensibility of the coherer.

This would explain Marconi's statement that at short distances a capacity consisting of a cubical tin box mounted at the top of the vertical wire, increased the sensibility of the coherer, but that he found it advisable to omit this capacity when telegraphing long distances.

It is also evident that one precaution must be taken when using a very sensitive coherer for wireless telegraphy. Since a steady current will produce coherer action if the potential difference between the ends of the coherer exceed what I have called the critical voltage, and since the lower this critical value is, the more sensitive will be the coherer for long-distance work, care must be taken that the electromotive force of the battery which operates the relay in Marconi's arrangement, shall have a value less than the critical value for the coherer used.

If an electromotive force of about the critical value is used, we might expect to notice a confusion of the signals from the sending station due to the local circuit through the coherer producing an independent and quite irregular lowering of the resistance.

Everyone who has attempted wireless telegraphy has probably been annoyed by extra " clicks " of the instrument. I believe it is in many cases attributable to the cause just mentioned, in which case it could be done away with by so choosing critical potential of coherer, electromotive force of battery in coherer circuit, and sensibility of the relay, that the first should be higher than the second, while the third should be great enough to permit of a small absolute value of the other two.

In conclusion, I should like to take this opportunity to express my thanks to my colleague, Dr. Karl Guthe, for his kindly interest in the present investigation, and for numerous helpful suggestions.

POLARIZATION AND POLARIZATION-CAPACITY OF ELECTROLYTIC CELLS.

BY K. E. GUTHE AND M. D. ATKINS, University of Michigan, Ann Arbor, Mich.

In a paper before this section last year[1] Dr. Guthe described a method for measuring the resistance of electrolytic cells, making use of Bartoli's empirical formula. If we suppose with Wiedeburg that this formula correctly expresses the polarization as a function of the time during which the current flows through the cell, this method becomes applicable also to measuring the "Initial Polarization capacity" of cells, even where this is very large. By "Initial Polarization capacity" is meant the value which C has in the Kohlrausch formula,

$$(I) \quad p = \frac{1}{C} \int i\,dt,$$

when t becomes zero.

By proper arrangement of the apparatus, as described in the paper mentioned above, the Bartoli formula reduces to the following form,

$$(II) \quad p - r = P(1 - e^{-Ki})$$

where p is the apparent, and r the real, resistance; P, a so-called "maximum polarization;" and K, a constant depending upon the electrodes.

When the current is large the apparent is equal to the real resistance, i. e., $p = r$.

When the current is zero,

$$(III) \quad p - r = KP,$$

that is, the curves for apparent resistance, plotted as a function of the current, cut the r axis at a distance KP above the real resistance. The relation of this value to the initial polarization capacity is an interesting one.

Putting the Kohlrausch formula above in the following form,

$$(IV) \quad C\frac{dp}{dt} = i;$$

and the Bartoli formula in the form,

$$(V) \quad p = P(1 - e^{-ait}),$$

we derive the polarization capacity at once as

$$(VI) \quad C' = \frac{e^{-ait}}{Pa}$$

or the initial capacity, as defined above, as

$$(VII) \quad C = \frac{1}{Pa}$$

this, using equation (II), becomes

$$(VIII) \quad C = \frac{t}{KP}$$

as the pendulum apparatus admits an easy determination of t, and K and P can be computed from the curves, it becomes easy to determine the polarization capacity for unit surface, for any electrode.

[1] Phys. Rev., vii, p. 193 (1898).

By investigations upon various combinations it has been possible to reach some conclusions in regard to the contradictory results reached and difficulties met, by other investigators.

There is, first of all, with each combination studied so far, a marked increase, in the range of the apparent resistance, which results from mere standing of the electrodes in the solution. This was found with $CuSO_4$ solution between copper electrodes; $AgNO_3$ between silver; $ZnSO_4$ between zinc; and H_2SO_4 between platinum. Since K and P both increase, a decrease occurs in the initial polarization capacity. Observers who have found such a decrease are, Lützen, Oberbeck, Gordon, Lowenstein, Wien and others. Obviously, if comparisons are to be made of results obtained by different observers, account must be taken of this time change. The cells showing least change of this sort were those set up of zinc sulphate solution, between electrodes of zinc amalgam, prepared by dissolving a small amount of C.P. zinc in pure mercury. That this time change is not an increase in the actual resistance, due to "films of badly conducting material" formed chemically within the cells, is very evident from the results obtained; since all the curves for a given series start at the same point with the large currents; while showing a wide increase in the range of the resistance.

As an example of this change the following series, taken with copper sulphate between copper electrodes, will serve for the purpose of this abstract:

			Apparent Resistance Changes.		
Electrodes in solution 1 hour from			5.50 ohms to		5.64
" " " 2½ hours "			5.50 " "		8.12
" " " 24 " "			5.50 " "		13.90

Amalgamating zinc electrodes, or electroplating copper ones, decreases the rate of this time-change by at least one-half. Shaking the liquid electrodes brings the curve back to nearly the same position as the first one taken. The results convince the observers that the change is one of the electrode, rather than the solution, also that it is not an increase of resistance. Absorption of the metal ions of the solution by the electrode may be the cause of this change.

A second marked disturbing factor was the decrease in apparent resistance caused by sending large currents through the cell. In every case studied this change was met with; again, least with liquid electrodes. An electrode consisting of a silvered glass mirror showed, after a single swing of the pendulum, with a current of 0.5 ampere, flowing for 0.2 second—a drop of 45 per cent. in the resistance, as measured just before, with a current of 0.01 ampere.

Deposited granular metal, by increasing the area of the electrodes, may produce this change.

In view of these changes results obtained with electrodes of varying areas, could not be expected to accord exactly with the demands of the formula. In all cases, however, larger areas gave flatter curves, while

many groups of curves were taken in which the inverse proportionality of areas and values for KP was nearly satisfied.

It is hoped that by using liquid electrodes, and by investigating the causes of these disturbing influences with mirror electrodes, more light may be shed upon this very interesting subject. The investigation will be continued along these lines.

CURRENT AND VOLTAGE CURVES IN MAGNETICALLY BLOWN ARC AND IN ALUMINUM ELECTROLYTIC CELL. BY REGINALD A. FESSENDEN, Western University of Pennsylvania, Allegheny, Pa.

It was shown that placing an alternating current arc in a strong magnetic field makes the current wave flat topped and increases the efficiency.

Curves of voltage and current taken between different points in an aluminum cell show that polarization and not pure resistance effect is due to formation of Al_2O_3, but that change of phase also takes place as the film acts as the dielectric of a condenser. The efficiency is low.

SOME NEW APPARATUS. TACHOMETER, CHRONOGRAPH, DATA COLLECTOR. INDUCTION COIL. BALANCE FOR STANDARDIZING AMPEREMETERS. STANDARD OF INDUCTION. BY REGINALD A. FESSENDEN, Western University of Pennsylvania, Allegheny, Pa.

Very brief description of instruments, touching only on main points. Instruments exhibited.

MEASUREMENT OF MAGNETISM IN IRON AND RELATION BETWEEN PERMEABILITY AND HYSTERESIS. BY REGINALD A. FESSENDEN, Western University of Pennsylvania, Allegheny, Pa.

Points out the difficulties met with in measuring permeability from scale on sheets and eddy currents in solid bars, when the ring method is used ; also the causes of error in using a permeameter. Proof is offered that the formula for the pull of magnetized rod in permeameter should be $S(B^2—BH)/8\pi$ instead of $S(B^2—H^2)/8\pi$. Description of new method, and of formula showing relation connecting permeability and hysteresis.

Instrument was shown, and diagrams and tables.

POLARIZATION AND INTERNAL RESISTANCE OF COPPER VOLTAMETER. BY B. E. MOORE, University of Nebraska, Lincoln, Nebr.

The polarization of electrolytic cells by means of the direct current is usually measured upon an independent circuit after the polarizing current is removed. The change from the one circuit to the other can not be made instantaneously and the polarization actually measured is less than the maximum. This has its effect upon the apparent interval resistance, r, which is calculated from the formula $r = \dfrac{E - P}{i}$, where E, P, and i are the applied electromotive force, polarization, and current respectively. Values of r thus obtained decrease enormously with increasing i. The experiments indicate that this arises from a failure to measure the maximum of P. This is shown by measuring P at different time intervals after the removal of the current. The time intervals vary from 0.00006 to 0.045 of a second. The results indicate that constant resistance for all current strengths would probably be obtained if the polarization could be measured in zero times after the removal of the polarizing electromotive force.

Measurements were made with the condenser and ballistic galvanometer, and the circuits were manipulated by a swinging pendulum. The smallest time interval is very near the time required to charge the condenser through the apparent electrolytic resistance. Therefore, shorter time intervals, though desirable, can hardly be obtained by the condenser method. However greater accuracy in the determination of the shortest time interval is possible and desirable.

The observations are limited to the copper voltameter with different sized plates.

CONCERNING THE FALL OF POTENTIAL AT THE ANODE IN A GEISSLER TUBE. BY CLARENCE A. SKINNER, PH.D., University of Nebraska, Lincoln, Nebraska.

Observations were made with a Geissler tube, cylindrical in form (length 20 cm., diameter 4 cm.) provided with disk electrodes (diameter about 2 cm.).

The results given below were obtained in nitrogen gas (pressure 0.6 to 3 mm.), through which was passed a constant electric current from a storage battery of 600 cells.

The fall of potential at the anode was measured by a quadrant electrometer as the difference of potential between the anode and a movable platinum wire which could be approached to contact with the anode. So far as it can be proved by this method the fall of potential at the anode is of the nature of a sudden "drop" between metal and gas.

In the immediate neighborhood of the anode the gas is a perfect conductor, the potential gradient is zero. The space of perfect conductivity

may extend about 2 mm. from the anode but its extent decreases rapidly with increasing gas pressure. Outside this space the potential gradient rises more or less rapidly to a constant value throughout the unstriated positive column, or luminosity.

The fall of potential at the anode (*a*) is independent of the current density, be the gas pure or impure, the anode clean or tarnished ; (*b*) increases slowly with increasing gas pressure ; (*c*) increases rapidly as the anode becomes tarnished ; (*d*) is sensibly influenced by impurities ·in the gas ; (*e*) for all metals with polished surface tested, lies between eighteen and thirty-five volts at 1 mm. gas pressure ; whereby, of two metals, the one which has the smallest fall of potential as cathode shows the greater as anode.

Further it was found that the greater the fall of potential at the anode the greater the distance from the anode in which the gas was rendered a perfect conductor, and also the slower the potential gradient outside this space rose to its constant value for the positive column.

(Diagrams were used.)

THE EQUIPMENT AND FACILITIES OF THE OFFICE OF U. S. STANDARD WEIGHTS AND MEASURES FOR THE VERIFICATION OF ELECTRICAL STANDARD AND MEASURING APPARATUS. BY FRANK A. WOLFF, JR., Office of Standard Weights and Measures, Washington, D. C.

The paper briefly outlines the steps so far taken by the Office of Standard Weights and Measures for the official verification of electrical standards and measuring apparatus submitted by institutions, the trade and by individuals. A description is given of the preliminary standards of reference adopted, the methods of comparison employed and the accuracy obtainable as shown by comparisons already made.

To fix the value of the preliminary standards of resistance pending the construction of a number of mercury units, frequent comparisons are to be made of the present standards with each other, with mercury copies the construction of which is to be taken up immediately and with new coils known to the highest degree of precision in terms of the Reichsanstalt mercury units and therefore in terms of the B. A. coils. The unit of electromotive force will similarly be fixed by the construction from time to time of new Clark and cadmium cells from specially purified materials, by the frequent intercomparison of the old ones of each type with each other and by the determination of the ratio from time to time of the Clark to the cadmium cells of the office.

The object of the paper is mainly to announce to the members of the association in particular and to the public in general this enlargement of the scope of the work and its policy to guarantee the highest accuracy attainable at present.

In this policy it is hoped that the office will be encouraged and sustained by the association.

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AN EXPERIMENTAL TEST OF THE ACCURACY OF OHM'S LAW. BY FRANK A. WOLFF, JR., PH.D., Office of Standard Weights and Measures, Washington, D. C.

By means of a very simple device a resistance is measured by a very weak test current while at the same time an additional current, varied between zero and any desirable limit, is passed through the resistance measured.

'Preliminary results indicate that if the resistance be expressed as a function of the current density in the following form :

$$R_c = R_o \left\{ 1 + h\left(\frac{c}{s}\right)^2 \right\}$$

then h cannot be greater than 1/600,000,000, the current density being expressed in amperes per sq. cm. The author believes he can reduce the above to 1/30 of the above by a choice of a more suitable material for the resistance coils.

MARCH WEATHER IN THE UNITED STATES, WITH SPECIAL REFERENCE TO THE MIDDLE ATLANTIC STATES. A STUDY OF THE RELATIONS EXISTING BETWEEN THE MEAN ATMOSPHERIC PRESSURE AND THE GENERAL CHARACTER OF THE WEATHER DURING MARCH. BY OLIVER LAUARD FASSIG, Johns Hopkins University, Baltimore, Md.

Assuming the average atmospheric pressure at the earth's surface at sea-level to be equivalent to about thirty inches of mercury, there is a band of relatively high pressure between latitudes 30° and 40° north and south of the equator at all seasons, and a band of relatively low pressure in the equatorial regions and in the north and south temperate zones. This distribution of pressure is the result of difference in temperature between the equatorial region and regions to the north and south, and of the revolution of the earth about its axis. It is well defined in the Southern Hemisphere, where surface conditions are quite uniform, being mostly a water surface. In the Northern Hemisphere, we have the large continental masses which introduce another factor into the problem. During the winter months there are large areas of high atmospheric pressure over the continents, and areas of low pressure over the relatively warmer oceans. During the summer months conditions are reversed ; the continents are relatively warmer than the oceans along the same latitudes, and the areas of high pressure are found over the oceans, and the low areas over the continents. Hence, at the earth's surface, there is a flow of large masses of air from continent to ocean in the winter months, and from ocean to continent in the summer months. These conditions are most marked in January and July ; during intermediate months the areas of high and low pressure occupy intermediate positions.

The problem is to find the positions of these areas of high and low

pressure during March and to determine their influence upon the general character of the weather of the month.

The pressure conditions during March were studied for all years from 1877 to 1899 over the United States ; also for some of these years over the entire Northern Hemisphere. For the United States the monthly mean temperature and rainfall conditions were charted in connection with the monthly mean pressure distribution, and their relations noted. March was found to be characterized by three types of pressure distribution : (1) *The normal type*, with an area of high pressure off the southeast Atlantic coast, and another of about equal strength to the north and west of the upper lakes, resulting in winds alternately from a southeasterly and a northwesterly direction in the Middle Atlantic States, with the strong contrasts in temperature so characteristic of March weather. (2) *The cold type* with a relatively stronger development of the area of high pressure in the northwest, and hence with prevailing winds from the northwest, or cold winds. (3) *The warm type*, with the Atlantic area of high pressure in control of the winds; hence southeasterly or warm winds prevail.

The cyclonic storms with accompanying rainfall which pass across the United States, generally in a northeasterly direction, move through the trough of relatively low pressure between these two high areas; hence the direction of the storm is determined by the relative positions of these high areas of pressure.

(Charts and diagrams were used).

A New Spectrophotometer and a Method of Optically Calibrating the Slit. By D. B. Brace, University of Nebraska, Lincoln, Nebraska.

Description of new spectrophotometer and special features of same ; namely, (1) Compound prism and (2) Calibration of slit.

On Achromatic Polarization in Crystalline Combinations. By D. B. Brace, University of Nebraska, Lincoln, Nebraska.

Conditions for achromatism of combinations of crystalline plates, and an experimental study of these conditions in mica, selenite, quartz, Iceland spar arragonite ¼ λ combinations.

On the Nature of Electricity and Magnetism and a Determination of the Density and Elasticity of the Ether. By Reginald A. Fessenden, Ph.D., Western University of Pennsylvania, Allegheny, Pa.

Fourier's idea of dimensions, as shown by his definitions, how these

have been departed from by later authors, in suppressing dimensions, thus making the formulae merely a question of units. Restoration of Fourier's ideas, and the application of them to the problem of the solution of the nature of electricity.

It is shown that all electrical phenomena whose laws can be formulated in relation to mechanical quantities are contained in the following three equations :

1. $q/b = k/u$
2. $k/u = T/L$
3. $qb = ML/T$
4. $qu = Z$

Having but three equations and four unknown quantities, ($q =$ quantity of electricity, $b =$ quantity of magnetism, k = specific inductive capacity and $u =$ magnetic permeability) we cannot directly solve. Putting however qu equal to an unknown term, Z, we may obtain all possible theories of electricity, including those already proposed by Lodge, Kelvin, Heavyside and others. For example, If Z be put equal to LT then k becomes a density, u the compliancy, etc. as given on one of Heavyside's theories. For solution we must get a fourth equation.

Sketch of work extending over a number of years in a search for this fourth equation. Discovery of relation between velocity of sound and electric conductivity. Other relations discovered, but none giving new equation.

Williamson's proof that either k or u must be a density. This proof incompleted. Proof made complete and same result obtained by two other methods. How to decide which is the density.

We have relations between q and b, and k and u. We must therefore look for relation between q and u or b and k; $i. e.$, between H and u and V and k. This leads us to the following conclusion, through the above three equations.

1. '' Either k varies inversely with V or u with H.''
2. '' The coefficient which varies inversely with the corresponding intensity is a compliancy.''

This gives a touchstone for detecting which of the two theories is correct. It is then shown that k does not vary inversely with V, but that u does with H. Therefore k is a density and u a compliancy.

Part 2 contains additional proofs. Shows that the effects of stress, permanent strain, change of composition, and elastic properties all agree with this theory and give the results predicted by it. It is shown that all properties, magnetic properties of iron, can be expressed by two coefficients, and it is shown that the hysteresis coefficient is equal to one of these multiplied by a constant.

It is also shown that there is a simple relation between the density of substances and their specific inductive capacity, and that we are thus enabled to determine the density of the ether. This is done. It is also

shown that the variation of density with temperature and with change of state follows the theory.

Part 3 contains an account of the experiments made to establish the relation between H and *u* and of the precautions taken to avoid error. Curves are given showing the results.

ADVANCES IN THEORETICAL METEOROLOGY.   BY CLEVELAND ABBÉ, U. S. Weather Bureau, Washington, D. C.   [Read by title.]

LOCATION OF SMOKELESS POWDER DISCHARGE BY MEANS OF COLORED SCREENS.   BY REGINALD A. FESSENDEN, PH.D., Western University of Pennsylvania, Allegheny, Pa.

The spectrum of the discharge gives a bright band in red. The diffused light in landscape is very weak in red. Therefore a colored screen which transmits only the red will markedly increase the contrast between the flash and the trees, and so enable it to be easily detected.

Specimens of screens shown.

A METHOD FOR THE STUDY OF PHOSPHORESCENT SULPHIDES.   BY FRED. E. KESTER, Ohio State University, Columbus, Ohio.

The sulphide under observation was in the form of a coating upon the convex surface of a small vertical cylinder, about three cm. in diameter; the cylinder was rotated at various speeds about its own axis. The coating was illuminated by monochromatic light on one side, and observed by means of a spectrophotometer as it passed the slit of the collimator.

In order to determine the intensity of the illuminating light, a modified form of the Crook's radiometer was used (form devised and used by Ernest F. Nichols, in Berlin).

In addition to the description of apparatus the paper contains a discussion of observations made on calcium sulphide.

ACCIDENTAL DOUBLE REFRACTION IN COLLOIDS AND CRYSTALLOIDS.   BY B. V. HILL, M.A., Denver, Col.

The apparatus consisted of two parallel cylinders external to each other, rotating in the same direction, between which the light passed. The solutions surrounded these cylinders in a chest. The light was examined by a half-shade polariscope. The sensibility was 100 to 200 times as great

as obtained by other observers, except by Mr. Almy, who used the same apparatus, and a sensibility twice as great as the latter was obtained. Sunlight was used and absorbing solutions made according to Landolt's formulæ, gave sufficiently homogeneous light. Solutions of gum arabic, hyposulphite of soda, sugar and gelatine were examined. The study is limited principally to the latter substance.

The effect obtained always depends upon the way in which the solutions are treated and made. In gum arabic the relative retardation is proportional to the speed but not directly so to the concentration. In hyposulphite of soda the effect probably arose from chemical action upon the nickel-lined chest. No relative retardation was obtained in sugar solutions, though a sixty per cent. solution was examined.

A study of the data for gelatine leads to the following conclusions: For very small velocities the relative retardation increases with the speed, but not in proportion to it. At a certain point the elastic limit seems to be reached. Beyond this the double refraction decreases, and in some cases ultimately becomes negative. In some very dilute solutions the breaking down takes place at too small a velocity to be observed, and the effect decreases with the speed. The rate of decrease of the double refraction is less for dilute solutions than for the stronger ones. For a one-tenth per cent. solution the double refraction becomes independent of the speed. These observations were all taken at about the ordinary temperature of the room. The diminution falls off so very rapidly with increased temperature that it was impossible, with the present apparatus, to study carefully the temperature effect. At about 27° or 28° C. the double refraction becomes constant for all speeds. At this temperature the double refraction was found to be proportional to the concentration. This is possibly connected with the elasticity of jelly found by Mauer to be proportional to the concentration. Double refraction and depolarization effect, always present at lower temperatures, cease abruptly at 33°, the melting-point of jelly. Upon cooling a solution to this temperature, the relative retardation was found nearly proportional to the speed. A 0.3 per cent. solution which cooled from 35° to 30°, in twenty-four hours more than doubled the relative retardation in that time. Fauss found the same for the elasticity. A solution containing about 16⅔ per cent. of gelatine, and another containing 0.5 per cent. gelatine, were subjected to static strains. The former gave a maximum retardation of 0.28 λ, the latter of 0.0028 λ. When a greater strain was applied to the latter, the double refraction suddenly disappeared, as if the substance had ruptured. Static strain applied to Canada balsam showed that the double refraction diminished, unless the strain was applied continuously, as if the particles were viscous, instead of being rigid, and were slowly sliding over one another.

The action of jelly in water below the former's melting-point, indicates a radical difference between colloids and crystalloids, contrary to the conclusion of Nernst, drawn from osmotic considerations. In a solution as dilute as one-hundredth per cent., the action reminds one of an elastic

solid. Whereas in a crystalloid cane-sugar solution, six thousand times as concentrated, no effect is observed. It seems doubtful, therefore, whether such a combination of jelly and water can be called a solution in the sense in which that term is usually accepted. The term, "solid solution," would also have to be given wider meaning to cover it.

NOTE ON THE AGE OF THE EARTH. BY REGINALD A. FESSENDEN, PH.D., Allegheny, Pa.

A note on the physical constants which enter into the determination of the age of the earth from radiational phenomena, and a discussion of the most probable value of the emissivity constant used in Lord Kelvin's calculations.

A BOLOMETRIC STUDY OF THE RADIATION OF A BLACK BODY BETWEEN 600° AND 1100° C. BY CHARLES E. MENDENHALL, PH.D., Williams College, Williamstown, Mass.

The black body consisted of a cylinder of iron or copper, heated in a furnace. The radiation was allowed to pass out through a slit in the side, and examined by a rock-salt spectroscope similar to that of Langley, at Allegheny. The resulting normal energy curves are plotted and discussed.

A BOLOMETRIC STUDY OF THE RADIATION OF AN ABSOLUTELY BLACK BODY. BY F. A. SAUNDERS, PH.D., Haverford College, Pa.

The object of this investigation was to construct a body which would radiate in a manner similar to a theoretically " absolutely black " body, and to measure the amount of energy belonging to each wave-length of this radiation and its variation with the temperature. The " black body " consisted of the space enclosed within the walls of a metal cylinder, blackened internally, with a small slit in its side to allow a certain amount of the radiation existing within the enclosure to pass to the measuring apparatus. The latter was a spectrobolometer similar to the form used by Langley, equipped with a rock-salt prism and lens ; and the galvanometer used with the bolometer was a low resistance four-coil,one of very high sensibility.

The range of temperature of the black body was from 578° C. to 100° C. At these two points and at five intermediate ones, " energy curves " were taken, and the results show that these in the main are of the form required by the formula lately proposed theoretically by Wien, and denied experimentally by Paschen.

ON THERMODYNAMIC SURFACES OF PRESSURE-VOLUME-TEMPERATURE FOR SOLID, LIQUID, AND GASEOUS STATES. BY FRANCIS E. NIPHER, Washington University, St. Louis. [Read by title.]

The paper deals with two classes of substances : those which contract,

and those which expand on solidifying. The conditions of pressure, volume, and temperature in the gaseous vapor and solid condition, will be shown by charts of those surfaces representing projections on the V. T. plane, of lines of equal pressure. The conditions of $\left\{\begin{array}{l}\text{vaporization}\\\text{liquefaction}\end{array}\right\}$ and of $\left\{\begin{array}{l}\text{fusion}\\\text{solidification}\end{array}\right\}$ are brought out, and also the condition of $\left\{\begin{array}{l}\text{sublimation}\\\text{vapor solidification}\end{array}\right\}$. The two critical temperatures, gas-vapor-liquid and solid- $\left\{\begin{array}{l}\text{fusible solid}\\\text{solidifiable liquid}\end{array}\right\}$ -liquid are discussed, and the significa-tion of the triple point pressure in limiting the condition where the liquid condition is impossible is discussed.

ON THE ESCAPE OF GASES FROM THE PLANETS ACCORDING TO THE KINETIC THEORY. BY S. R. COOK, A.M., M.S., University of Nebraska, Lincoln, Neb.

The purpose of the paper is to apply Maxwell's distribution of veloci-ties to the escape of gases from planets.

The number of molecules of a given gas that will escape under given conditions from a planet in time T is,

$$N = \frac{T}{t} \cdot \frac{1}{2} (1-\cos\theta)n'4\pi R^2 m\lambda K.$$

When t = time of mean free path of the molecule.

R = distance from the center of the planet to the limit of the atmosphere.

λ = mean free path.

m = ratio of the critical velocity to the mean velocity.

n' = number of molecules per cubic centimeter of the gas.

K = the number of molecules having the critical velocity.

For the earth this formula is computed under the following four conditions:

1. For a spherical shell at the earth's surface at a mean temperature of 5° C.

2. For a spherical shell 200 km. from the earth's surface at a tempera-ture of 66° C.

3. For a spherical shell 20 km. from the earth's surface at a tempera-ture of 66° C.

4. For a spherical shell 50 km. from the earth's surface at a tempera-ture of 180° C.

The result for an atmosphere of hydrogen for T = 10,000,000 years in cc. at normal pressure, and temperature are, for condition 1, $33.04(10)^{15}$; condition 2, 23.58; condition 3, $54.28(10)^5$; and for condition 4, $43.5(10)^{-8}$. For an atmosphere of helium the results obtained for the

four conditions are $10.34(10)^{-4}$; $22.16(10)^{-24}$; $26.73(10)^{-23}$; $91.6(10)^{-86}$.

The results obtained for the earth were then applied to the moon and several of the planets. It was found that a hydrogen atmosphere would leave the moon at $-256°$ C. Mercury at $-209°$. Venus at $20°.5$. Mars at $-195°$. The earth at $291°$ C. An air atmosphere would leave the moon at $-10°$ C. Mercury at $894°$. Venus at $5031°$. Mars at $1139°$, And the earth at $9937°$. An atmosphere of carbon dioxide would leave the moon and above planets at $274°$ C., $1371°$ C., $7403°$ C., $1807°$ C., $14447°$ C., respectively.

ON DIFFERENTIAL DISPERSION IN DOUBLE REFRACTING MEDIA.
BY E. J. RENDTORFF, University of Nebraska, Lincoln, Neb.

RELATION OF MAGNETIZATION TO THE MODULUS OF ELASTICITY.
BY JAMES S. STEVENS, The University of Maine, Orono, Me.
[Read by title.]

ON FLUTINGS IN THE KUNDT SOUND TUBE. BY S. R. COOK, A.M., M.S., University of Nebraska, Lincoln, Nebraska.

The purpose of the experiments set forth in the paper was to obtain some experimental data, which would verify Walter König's equations for the forces which act in a sound tube to produce flutings.

Some preliminary experiments were made : as producing flutings by a direct sound wave ; testing the effect of various materials, as magnesium carbonate, lycopodium, amorphous silica, cork dust, magnesium, anthracene, ammonium chloride (fumes), sand, aluminum, iron, brass, copper, coin silver, and platinum filings, timothy, blue grass, red-top and petunia seed, when used as fluting materials ; upon the vibration of the laminæ with a microscopic examination of the formation of the laminæ, and the influence of the density of the medium on the flutings.

If the x axis is normal to, and the y axis parallel to, the direction of the stream lines, König gives the equation for the forces x, y, z, between two spheres whose distance apart is r.

$$x = \frac{3}{2} \frac{\pi d R^3 R_1^3 W^2}{r^4} \sin \theta (1-5 \cos^2 \theta).$$

$$y = 0.$$

$$z = \frac{3}{2} \frac{\pi d R^3 R_1^3 W^2}{r^4} \cos \theta (3-5 \cos^2 \theta).$$

d = the density of the medium.

R and R_1 = radii of spheres ; W, the velocity of the

stream, and θ the angle which the line of centers of the spheres makes with the direction of the stream lines.

When $\theta = \dfrac{\pi}{2}$,

$$x = \frac{3}{2} \frac{\pi d\, R^3 R_1^3 W^2.}{r^4}$$

When $\theta = \pi$,

$$z = 3\, \frac{\pi d K^3 R_1^3 W^2}{r^4}$$

These equations give repulsion parallel to and attraction perpendicular to the stream lines.

In my determinations with air and carbon dioxide, and with air and hydrogen, R, R_1, r, and W may be considered constant. If x and z represent the forces of attraction and repulsion of air, x_1 and z_1 the same for carbon dioxide, we have,

$$\frac{x}{x_1} = \frac{z}{z_1} = \frac{d}{d_1},$$

when d = density of air, and d_1 = the density of carbon dioxide.

In experiments with air and carbon dioxide when x was made eqnal to x_1, and z was made equal to z_1, the ratio of the densities varied from 0.42, the lowest for magnesium carbonate, to 0.82 for sand. The ratio of the density of hydrogen to air for the same effect was 5.85.

DIELECTRIC STRENGTH OF OILS. BY THOMAS GRAY, Rose Polytechnic Institute, Terra Haute, Ind.

At the last meeting of this association I presented an account of some experiments on the dielectric strength of insulating substances. I then mentioned that the results for oils were somewhat uncertain, and that it had been found difficult to form any very decided opinion as to whether the dielectric strength of a layer of oil is or is not proportional to the thickness of the layer During the past spring Mr. C. B. Keyes, a member of the graduating class at the Rose Polytechnic Institute, has continued the experiments on oil. Mr. Keyes has directed his attention largely to the questions of proportionality of strength to thickness, and the effect of filtering through drying materials like calcium chloride. The difficulties experienced last year, due to the variations of the dielectric strength under continued electric stress, were again met with. It was found that filtering through calcium chloride or leaving the oil for a day or two, mixed with that substance, did not remove the trouble, and almost always reduced the strength considerably. By making a large number of measurements in cycles of varying thickness of layer, and then averaging the results considerable evidence was accumulated, which went

to show that the dielectric strength per centimeter thickness is greater the thinner the layer tested. Oil seems therefore to agree with gases and most solids in this respect. The subject is not by any means closed, but I give below a few of the average results as an indication of the nature of the evidence obtained. I should perhaps remark again, as I did last year, that the absolute value of the dielectric strength is largely a function of the length of time during which the oil has been subjected to electric stress. A considerable number of oils were tested, including several of those the result for which were quoted in last year's paper. Many of the samples gave results of a very erratic character. It seems probable that there is a gradual change in the dielectric properties of these liquids while under stress. Possibly the whole difficulty is due to a species of polarization which may be most marked close to the plates.

In the following short table the numbers in the headings are the thickness of the layers tested; the first column the name of the oil, and the numbers in the body of the table the dielectric strength in Kilovolt's per centimeter.

Kind of oil.	1	2	3	4	5	6	7	8	9	10
Vaseline	131	120	109	103	105	95	96	91		
Paraffin 28 G	91	81	76	71	73	70	69	70	72	
Paraffin 26 G	101	89	85	79	79	71	66	64	62	60
N.W.V. C. 29 G.	81	79	74	72	69	62	63	62	60	

Natural West Virginia Crude Oil from 29-30 gravity.

SOME UNEXPECTED ERRORS IN WATT-METER MEASUREMENTS. BY THOMAS GRAY, Rose Polytechnic Institute, Terre Haute, Ind.

A few weeks ago a new wattmeter was sent to the author of this paper with the request that it be standardized as it had been found to indicate higher than some older instruments belonging to the same company. The first tests showed the instrument to be practically correct in its indications. After standing over night the weather being very wet another test was begun. It was found that for alternating currents the resistance in series with the potential coil had considerably diminished while for continuous current it was nearly the same as on the previous day. After further exposure to the moist atmosphere the resistance was found to be still smaller and that it was diminished below the normal even for continuous current. The resistance boxes were then placed in a closed case containing calcium chloride and left for about two days. After this the resistance was found to be normal. The cause of the error was due to hygroscopic action in the material carrying the resistance coils or in the varnish covering them; some attempts were made to reproduce similar defects in another instrument so as to use it for investigation of the effect on the instrument constant but this has not been very satisfactorily done so far.

A number of experiments were made on a similar case; namely, the effect of putting a condenser on as a shunt to the additional fine wire resistance, some resistance being put in series with the condenser and varying amounts of electromagnetic inertia added to the fine wire circuit.

Let $L_2 R_2$ be the coefficient of induction and the resistance of the circuit in which the activity is to be measured; C the current through the fine wire coil of the wattmeter and R its resistance; L_1 the coefficient of induction and R_1 the resistance of the multiplier coil; K the capacity of the condenser and R_3 the resistance in series with it. Then the apparent watts are given by the expression

$$\frac{E^2 \sqrt{(1-KL_1 w^2)^2 + K^2(R_3 + R_1)^2 w^2}}{\sqrt{(R_2^2 + w^2 L_2^2)}\left[\{ w^2(R_3 + R)L_1 K - (R_1 + R) \}^2 + \{ KRR_1 + L_1 + K(R_1 + R)R_3 \}^2 w^2\right]} \cos(\alpha + \beta + \gamma)$$

$E = e.m.f.$ amplitude and w the angular velocity corresponding to period of alternation

$$\tan \alpha = \frac{(R_3 + R_1)Kw}{1 - KL_1 w^2}, \quad \tan \beta = \frac{[K\{ RR_1 + (R_1 + R)R_3 \} + L_1]w}{w^2 KL_1(R_3 + R) - (R_1 + R)},$$

$$\tan \gamma = \frac{L_2 w}{R_2}.$$

Some experimental verification of the consequences of this formula were attempted.

(a) Putting $L_2 = 0$ and varying L_1 it was found that when L_1 was zero the effect of K was increase of deflection. When L_1 was increased above a certain limit K diminished the deflection the effect being less as R_3 is made greater. If L_1 be still further increased we find ultimately that the effect of K can be reversed by changing R_3.

(b) When L_2 is not zero but L_1 is zero the effect of K is usually to reverse the deflection; that is, the difference of phase becomes greater than 90°.

(c) L_2 and L_1 both finite. For low values of L_1 the effect of K was to increase deflection when $R_3 = 0$ but raising R_3 reversed effect. When L_1 was adjusted so as to give maximum deflection the effect of capacity was always a diminution of the reading.

The capacity used was one-third microfarad.

NOTE ON THE PREPARATION OF RETICLES. BY DAVID P. TODD, Amherst College, Amherst, Mass. [Read by title.]

Ordinary forms of ruled reticles may be made by the following method, but it is specially adapted to preparing reticles with curved lines, letters, and figures. These are often necessary in special astronomical and

physical investigations, and they cannot be made with a ruling engine conveniently, nor without large expense.

Briefly outlined the method is (1) to draft the required reticle with India ink on Bristol board, (2) to prepare a reduced negative of the same, and (3) to make a still farther reduced positive, on the thinnest glass available.

Use the old-fashioned wet collodion process, and calculate carefully the size of the positive, knowing the focal length of the telescope in which the reticle is to be used.

THE NATURE OF SPOKEN VOWELS, WITH REFERENCE TO THE THEORIES OF HELMHOLTZ AND HERMANN. BY E. W. SCRIPTURE, Yale University, New Haven, Conn. [Read by title.]

An account of some recent investigations, hitherto unpublished.

PRESSURE AND WAVE-LENGTH. BY JOHN FRED MOHLER, PH.D., Dickinson College, Carlisle, Pa. [Read by title.]

Continuing the work first reported to the A. A. A. S. at the Springfield meeting and published in the *Astrophysical Journal*, February, 1896, October, 1896, and in the *Johns Hopkins Circular*, February, 1896, I have investigated the behavior of the lines in the *spark* spectrum of cadmium and iron. Harschek and Mache (*Astrophysical Journal, ix, 5*), have shown that great pressure is produced in the spark. My results differ somewhat from their's, being obtained in an entirely different way. I have varied the capacity, pressure, and medium obtaining some interesting results. The work has considerable bearing on that of Prof. Wilsing of the Potsdam observatory (Sitz der K. P. Akad. der Wissenschaften, xxiv, 1899).

THE ATTENUATION OF SOUND AND THE CONSTANT OF RADIATION OF AIR. BY A. WILMER DUFF. Introduced by PROF. T. C. MENDEN-HALL. [Read by title.]

It is frequently stated that the intensity of sound propagated from a source varies inversely as the square of the distance from the source. This would, of course, necessarily be true were there no diminution of the energy of vibration ; but that some diminution must occur is obvious when we consider that the internal friction of air must cause some of the

energy of vibration to be frittered down into heat, and further that radiation and conduction from a compression of some of the heat produced by the work spent in producing the compression must decrease the energy of the following expansion, while a similar transference of heat to a rarefaction must tend in the same direction.

In a paper already published, the author has given a mathematical investigation of the propagation of spherical waves of sound, allowing for the effects of viscosity, radiation, and conduction. This investigation need not be repeated here, but the general result must be stated with a view to the understanding of the experimental work that will be described. It was found that the radial velocity of the vibrating medium is proportional to

$$E^{-mr}\left\{ \frac{n}{ar} \sin n \left(t - \frac{r}{a} \right) - \frac{1 + mr}{r^2} \cdot \cos n \left(t - \frac{r}{a} \right) \right\} \cdots (1).$$

In which n denotes the vibration frequency;

        a denotes the velocity of sound;

        r denotes the distance from the source;

and m consists of the sum of three terms depending on viscosity, conduction, and radiation, respectively; namely,

$$m = \frac{4}{3} \frac{n^2}{a} \mu^1 + \frac{\gamma - 1}{\gamma} \frac{n^2}{a} \nu + \frac{\gamma - 1}{\gamma} \frac{1}{a} \lambda \cdots (2).$$

μ^1 denoting the kinematic coefficient of viscosity of air,

ν "   "   conductivity of air,

λ "   "   constant of radiation according to Newton's law of cooling,

and γ "   "   ratio of the specific heat at constant pressure to that at constant volume.

From (1) it is seen that the motion at any point may be considered as the resultant of two simple harmonic motions, differing in phase by a quarter of a period; but the relative importance of these terms differs widely for different values of r. For large values of r the second component is quite negligible, and the motion is represented by

$$\frac{E^{-mr}}{r} \sin n \left(t - \frac{r}{a} \right).$$

At small distances, since m is essentially a small quantity, the term mr may be neglected in comparison with unity, and the resultant motion is represented by

$$\frac{E^{-mr}}{r} \sqrt{1 + \frac{a^2}{n^2 r^2}} \sin \left\{ n \left(t - \frac{r}{a} \right) - \phi \right\}$$

in which $\phi = \tan^{-1} \frac{1 + mr}{nr} a.$

Moreover, for small values of r, E^{-mr} does not differ appreciably from unity. Hence the law of intensity at small distances is

$$\frac{1}{r^2}\left(1 + \frac{a^2}{n^2 r^2}\right),$$

while that at large distances is

$$\frac{E^{-2mr}}{r^2}$$

In the previous paper the author described the results of an experimental method for finding the value of m. This method consisted essentially in observing the distance to which eight similar whistles blown under a definite pressure were audible and thus finding the smaller distances to which the whistles arranged in pairs were audible. The details of the work and the discussion of possible objections to the method need not be repeated here. Suffice it to say that whereas eight whistles should, according to the law of inverse squares, be audible just twice as far as two whistles, they were in reality only audible one-fourth farther than two whistles. From this the value of m was deduced. The values obtained from five sets of observations under widely different circumstances were (in C. G. S. units) 0.000034, 0.000040, 0.000043, 0.000049, 0.000045, the mean value being 0.000042. From this it may be deduced that the total energy of vibration decreases at about four-fifths of one per cent. per meter. Now as m is according to (2) the sum of certain multiples of the constants of viscosity, conduction, and radiation, whereof the constants of viscosity and conduction are well known while the constant of radiation has never been estimated either experimentally or theoretically, it is clear that we have the means of finding, roughly at least, a maximum value for the constant of radiation of air. It resulted that supposing for the small range of temperature involved Newton's law of radiation holds true, the constant for air in that law does not exceed 8.3 or that a mass of air at any given excess of temperature above the surrounding medium will, if its volume remain constant, fall to one-half of that excess in not less than one-twelfth of a second. This is stated as an upper limit for it is made on the assumption that the decay of intensity is due entirely to the causes enumerated and that any part not accounted for by viscosity and conduction must be due to radiation. Strong reasons were advanced for the belief that atmospheric refraction (which would cause a curvature of the lines of radiation) and internal reflection arising from a lack of homogeneity of the medium could not play a part of any importance under the circumstances of observation.

In a discussion of the preceding results which Lord Rayleigh has contributed to the *Philosophical Magazine* the ground is taken that the maximum value thus assigned to the constant of radiation must be much higher than its actual value and, if no part of the decay of intensity can be shown by further observations to be due to atmospheric refraction, a

further cause must be sought. This cause, Lord Rayleigh thinks, may be found in the time required for the kinetic energy produced by compression, which at first is purely translational, to become shared between the translational and rotational forms. This opens a new and far-reaching point in the kinetic theory of gases on which the present writer hopes to gain some light by experiments which he is preparing to undertake.

In the meantime it seemed well to attempt by an independent method to settle whether the rate of decay of intensity is really as high as the previous experiments seemed to show. The results obtained by this second method are still somewhat imperfect ; but, as an opportunity for repeating and extending the work may not present itself for a considerable length of time, it has been thought permissible to bring the results already attained to the attention of the association.

The sounds studied by this second method were produced as before by whistles blown under definite pressure, the observations being made at a very quiet part of the River St. John in New Brunswick. To obtain a variable standard with which to compare the sounds at different distances, a telephone transmitter was placed near the whistle and a receiver, connected with the transmitter through a line of constant resistance, was held so that its distance could be varied until the intensity of the sound heard directly was equal to that of the sound given off by the receiver. At first only a single whistle was employed, it being placed in a heavily padded box that could be alternately opened and closed, but afterwards it was found better to employ two whistles of the same form, dimensions, and pitch. One mounted in the open air acted as the source of sound to be studied, the other was enclosed with the transmitter in a heavily padded box and the two were sounded alternately. The whistles were sounded by an assistant on an isolated pier in the middle of the river, the whistles being about 25 feet above the level of the water. The observer was in a canoe which was allowed to float down stream from the pier and arrested at any portion by means of a cord attached to the pier, this cord also affording a means of determining the distance from the pier. The telephone line consisted of 1000 feet of twin-wire doubly insulated with rubber to protect it from the water, the whole line being always in circuit so that the resistance remained constant. Only one ear was used in making the observations, the other being filled with wool and covered closely by a heavy woolen pad.

In this method it was expected that the telephone would form a standard of the same pitch and quality as the source ; but, in making the observations, the ear became very sensitive to minute differences of quality between the sounds compared. For whistles of different pitch these differences were found to vary greatly and for only one particular pitch did the differences seem to disappear entirely ; namely, for a pitch of about 4000. It may be here noted, as a matter of independent interest, that this frequency of 4000 was afterward found by calculation to be also that of the free vibrations of the disk of the receiver, not the fundamental vibra-

tions but those of the first higher mode. Unfortunately this point was not foreseen and the range of the sounds tried probably did not include that corresponding to the fundamental vibrations of the disk. It seems probable that a telephone (with a carbon transmitter) transmits comparatively unmodified, only those notes which synchronize with one of the modes of vibration of the receiver. I am not aware that the point has ever been studied. For this particular note, the sound heard directly and that eminating from the receiver were at times so nearly identical as to be entirely indistinguishable when the receiver was held at the proper distance. In obtaining the distance of the receiver that distance was measured from the disk to the outline of the skull adjacent to the ear.

If, then, the mathematical investigation be correct, we can deduce the intensities at different distances from the receiver from the law

$$\frac{1}{r^2}\left(1 + \frac{a^2}{n^2 r^2}\right)$$

and are thus enabled to find the constant of decay m which enters the formula

$$\frac{E^{-2mr}}{r^2}$$

which, according to the mathematical investigation, expresses the law of intensity at large distances from the source. It would, of course, be a more satisfactory procedure to submit the law for short distances from the receiver to experimental verification. This the author has attempted to do by a method that need not be explained, for the only observations for that purpose that atmospheric conditions at the time permitted, a very rough set indeed, were found unfortunately so inconsistent among themselves that nothing positive could be deduced. Hence for the present, the former and less satisfactory method must be pursued.

Table I contains the results of two series of observations made by the above method. These two series were made on the same occasion but unfortunately a readjustment of the relative position of the transmitter and the whistle that acted on it was made between the two series, the purpose of this readjustment being to eliminate a slight difference of quality that seemed to exist between the direct sound and the telephonic sound.

TABLE I.

Distance in cm.	Relative intensity.
12000	16.4
18000	6.33
24000	1.84
15000	11.3
21000	4.98
27000	1.62

The effect of the readjustment evidently was to make the standard of slightly smaller intensity and hence to raise the succeeding estimates of intensity. Hence it should be possible to bring the two series into harmony by reducing the intensities of the second series in a constant proportion. Such a reduction has been made as indicated in the accompanying figure, the reducing factor being 0.75. With this reduction all the results are fairly well represented by a single curve which therefore forms the most probable representation of the variation of intensity within the range of observation. Taking it as such we derive from it

DISTANCES IN METERS

Table II, in the last column of which are found the values of m that would be necessary in order to reconcile the observed decay of intensity at each increase of distance with theoretical law.

TABLE II.

Distance	Intensity.	m
12000	16.4	
15000	9.0	0.000026
18000	5 4	0.000024
21000	3 30	0.000031
24000	1.90	0.000047
27000	1.17	0.000042

The mean of these values for *m* is 0.000034. If now we subtract the part of *m* due to viscosity and conduction, in this case 0.000002, we get 0.000032 as the part due apparently to radiation which is seen by (2) to be independent of pitch. Doing the same in the case of the shriller whistles (of vibration frequency 7000) used in the earlier experiments we get 0.000035. The two results seem in very good agreement indeed when the difficulties and differences of the methods are considered. This confirmation of the earlier result lends added weight to Lord Rayleigh's view that some cause of decay other than viscosity, conduction, and radiation may have to be sought.

The apparent increase of *m* with distance as shown in Table II would seem to point in the same direction, indicating a cause of decay that, if included in the mathematical analysis, would not lead to an exponential law. On the other hand, this apparent increase of *m* may be really due to imperfections in the application of the second method, possibly to an error in the method of determining the distance between the receiver and the ear, or to a lack of accuracy in the theoretical law for small distances. The author hopes to return to the subject at a later date.

ERRATA IN THE PRECEDING ARTICLE.

Following formula (1), the definition of *n* should read :

In which *n* denotes the vibration frequency × 2π.

Formula (2) should be written :

$$m = \frac{2}{3} \frac{n^2}{a^3} \mu' + \frac{\gamma-1}{\gamma} \frac{n^2}{2a^3} \nu + \frac{\gamma-1}{\gamma} \frac{1}{2a} \lambda$$

The curve was plotted from figures in which there was a slight systematic inaccuracy, but it conveys to the eye a quite correct impression of the connection of the quantities represented.

OPTICAL CALIBRATION OF THE SLIT OF A SPECTROMETER. BY E. V. CAPPS, University of Nebraska, Lincoln, Nebraska.

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ADDRESS

BY

F. P. VENABLE

VICE-PRESIDENT, AND CHAIRMAN OF SECTION C.

THE DEFINITION OF THE ELEMENT.

It is with hesitation that I enter upon so speculative a discussion as the nature of the elements and yet there are reasons why it should prove of great profit to draw the attention of this representative gathering of the chemists of America to this subject. We have nearly reached the close of the first century in which these elements have been the subject of experimental research. . The ingenuity and the patient labor of an army of workers have been directed toward the solution of the many problems connected with these elementary substances and the ultimate aim, the goal of all their striving has been the discovery of the properties and nature of the atom.

It is eminently fitting then that, as we stand at the threshold of the new century, we glance back along the road we have already come and take some count of the progress we have made. The quicksands of mere speculation must be avoided and yet the mental vision, the "scientific imagination," must be called into service in considering that which so far transcends our cruder actual vision as the incomparable atom itself.

There is another reason for considering the nature of the elements. At several times during the century a wider vision has made it necessary to re-cast the definition of the elements to accord with increasing knowledge. It would seem as if another such period of change were approaching. There may be need of a truer definition and how shall this be realized or

the new definition properly fitted unless the knowledge gained be summed up and appreciated.

The conception of an element among the Greek philosophers and the earlier alchemists was very different from the modern idea. This conception sprang from the theories as to the formation of the material universe. The elements were the primal forms of matter, seen only combined, impure, imperfect. They were the essences or principles out of which all things were evolved. In the four-element theory, which was so widely spread among the ancients, the fire, air, earth and water were not the ordinary substances known under these names, but the pure essences bestowing upon the fire and water their peculiar properties. These essences were not thought of as actual substances capable of a separate material existence and gradually the belief that a transmutation was possible between them sprang up. Thus, they themselves might be derived from some one of them, as fire or water. The Thalesian theory deriving all things from water was especially popular and was not completely overthrown until the modern era.

When later on the alchemists conceived of all metals as composed of sulphur and mercury, it was an essence or a spirit of sulphur and of mercury which they meant. Certain common characteristics as luster, malleability, fusibility, combustibility, etc., naturally led them to think of the metals as belonging to the same order of substances, containing the same principles, the relative proportions and purity of which determined the variations in the observed properties. Thus, the properties of the metals depended upon the purity of the mercury and sulphur contained in them, the quantities of them and their degree of fixation. The more easily a metal was oxidized on being heated the more sulphur it contained and this sulphur also determined its changeability. The more malleable it was the more mercury entered into its composition. If only something could be found which would remove the grossness from these essences, some unchanging all powerful essence which because of their search for it gradually became known as the philosopher's stone, then the baser metals might be transmuted into the noble gold where the sulphur and mercury were perfectly balanced and freed from all distempers.

As has been said, these principles entering, all or some of them, into every known substance, were supposed to be not necessarily capable of independent existence themselves. This was the view held by the followers of Aristotle. With the reaction against the domination of the scholiasts, other views began to be held. It was Boyle who first gave voice to these changed views in his " Sceptical Chymist" (1661). He defined elements as "certain primitive and simple bodies, which, not being made of any other bodies, or of one another, are the ingredients of which all those called perfectly mixed bodies are immediately compounded, and into which they are ultimately resolved." He, however, did not believe himself warranted from the knowledge then possessed, in claiming the positive existence of such elements.

But little attention was paid to the subject by subsequent chemists. The phlogistics were too much occupied with their theory of combustion and none could see the bearing of this question and its importance to exact science. Macquer, in his Dictionary of Chemistry (1777), words his definition as follows : " Those bodies are called elements which are so simple that they cannot by any known means be decomposed or even altered and which also enter as principles or constituent parts, into the combination of other bodies, which are therefore called compound bodies." To this he adds : " The bodies in which this simplicity has been observed are fire, air, and the purest earth." In all of this may be observed the resolution of observed forms of matter into primal principles, following the dream of Lucretius and the early Epicurean philosophers, a dream abandoned by the atomic school following, though largely holding to the same definition.

It was only when chemists began to realize that the mere observation of properties, chiefly physical, was not sufficient that the subject began to clear up and lose its vagueness. Black proved that certain substances were possessed of a constant and definite composition and fixed properties, unalterable and hence simple bodies or elements. Lavoisier finally cleared the way for the work of the nineteenth century by his definition that " an element is a substance from which no simpler body has as yet been obtained : a body in which no change

causes a diminution of weight. Every substance is to be regarded as an element until it is proved to be otherwise." With this clear definition to build upon, a rational system of chemistry became for the first time a possibility.

Thus, the elements were recognized as simple bodies because there were no simpler. They were not complex or compound. The distinction was clearly drawn between bodies simple, and bodies compound, and the name simple body has been frequently used as a synonym for element through a large part of this century. Naturally the question of simplicity was first settled by an appeal to that great arbiter of chemical questions, the balance. And quite as naturally many blunders were made and the list of bodies erroneously supposed to be simple is very long. All those whose weight could not be reduced were considered elementary. When, however, from such a body something of lesser weight could be produced its supposed simplicity was of course disproved.

This test for the elemental character has been clung to persistently and is perhaps still taught although it was long ago recognized that many of the elements existed in several different forms, a phenomenon to which Berzelius gave the name allotropism. One only of these could be the simplest and the others could be reduced to this one and rendered specifically lighter. With the discovery of this relation it should have been quite apparent that the old definition would no longer hold, but many years passed before chemists were made to feel that a new definition was necessary and adapted one to the newer knowledge.

The insight into what Lucretius would call the nature of things was becoming clearer ; the mental grasp upon those elusive atoms about which the old Epicurean reasoned so shrewdly was becoming firmer. Through what one must regard as the veil interposed by the earlier idea of the element the chemist began to grope after the constituent particle or atom. It must be borne in mind that the definition of the element was largely formulated before the resuscitation of the atomic theory by Dalton and the mental picture of the one has perhaps retarded the clearing up of the ideas concerning the other.

From the atomic point of view the element was next defined

as one in which the molecules or divisible particles were made up of similar indivisible particles. This afforded an easy explanation of allotropism as a change in the number of atoms in the molecule. As Remsen says, "an element is a substance made up of atoms of the same kind ; a compound is a substance made up of atoms of unlike kind."

Laying aside then all vaguely formulated ideas of essences, or principles, or simple bodies, or elemental forms, we found our present building upon the conception of the ultimate particle, be this molecule or atom.

As to this atom some clear conception is needed and here we come to the *crux* of the modern theories. The chemist regards this atom as a particle of matter and is unwilling to accept the theory of Boscovich that it is infinitely small and hence a mathematical point, nor can he admit that it is merely a resisting point and hence that all matter is but a system of forces. And yet it seems as though some authorities would lead up to such a conclusion.

While we need not consider these atoms as mere centers of forces, we are compelled to study them by the operation of forces upon them. What are called their properties have been studied and recorded with great care. These properties are evinced in the action of the forces upon matter, and the exhibition of force without matter cannot be admitted. This study of the properties has been the especial occupation of the century now closing and so the elemental atom has come to be regarded as a collection of properties. As Pattison-Muir puts it (Alchemical Essence and the Chemical Element, p. 31.) : "The name copper is used to distinguish a certain group of properties that we always find associated together, from other groups of associated properties, and if we do not find the group of properties connoted by the term copper we do not find copper."

These properties are exhibited by the action of a small group of forces. Perhaps we do not know all the forces, certain it is that we do not accurately know all of the properties but, to quote Pattison-Muir again, "the discovery of new properties always associated with the group of properties we call copper would not invalidate the statement that copper is always copper." The properties of an atom are either primary, in-

herent and as unchanging as the atom itself, or they are
secondary and dependent upon the influence of other atoms, or
varying with the change of conditions. To the first class
belong such properties as the atomic weight, atomic heat,
specific gravity, etc.; to the second, chemical affinity, valence,
etc. In all the study of the atom the distinction between these
should be carefully maintained in order that there may be clear
thinking.

There is no field of mental activity requiring more faith than
that of the chemist. He is dealing with the "evidences of
things unseen." He must not be content with the mere gathering
of facts but divine what he can of their deeper meaning. Few
chemists have had such insight as Graham into the significance
of even the simplest changes. He was not content with mere
surface observation. Even the commonest observed phenomena
were to him full of meaning as to the atoms and their "eternal
motion." Thorpe (Essays in Historical Chemistry, p. 219)
has drawn afresh the attention of chemists to the thoughtful
words of this great thinker. His mind was filled with the
fascinating dream of the unity of matter. "In all his work,"
says Adam Smith, "we find him steadily thinking on the
ultimate composition of bodies. He searches after it in follow-
ing the molecules of gases when diffusing; these he watches as
they flow into a vacuum or into other gases, and observes
carefully as they pass through tubes, noting the effect of weight,
of composition upon them in transpiration. He follows them
as they enter into liquids and pass out and as they are absorbed
or dissolved by colloid bodies; he attentively inquires if they
are absorbed by metals in a similar manner and finds remotest
analogies, which by their boldness, compel one to stop reading
and to think if they be really possible."

In his paper entitled "Speculative Ideas Respecting the
Constitution of Matter" published in the Proceedings of the
Royal Society in 1863, which Thorpe calls his "Confession of
Faith," he tells of his conception that these supposed elements
of ours may possess one and the same ultimate or atomic
molecule existing in different conditions of movement.

It is not possible for me, in the limits of this address to
array before you all of the various evidence which leads to the

belief that our so-called elementary atoms are after all but compounds of an intimate peculiar nature whose dissociation we have as yet been unable to accomplish. When properly marshalled it gives a very staggering blow to the old faith. Thorpe speaks of the "old metaphysical quibble concerning the divisibility or indivisibility of the atom." To Graham "the atom meant something which is not divided, not something which cannot be divided." The original indivisible atom may be something far down in the make-up of the molecule.

How shall the question as to the composite nature of the elements be approached? The problem has been attacked from the experimental side several times during the last half of the century but the work seems to have been carried on after a desultory fashion and was soon dropped as if the workers were convinced of its uselessness. The results, being negative, simply serve to show that no method was hit upon for decomposing the elements upon which the experiments were performed.

Thus, for instance, Despretz performed a number of experiments to combat Dumas' views as to the composite nature of the elements.

Despretz made use of the well-known laboratory methods for the separation and purification of substances. Such were distillation, electrolysis, fractional precipitation, etc. Such work was quite inadequate to settle the question, as Dumas had pointed out that unusual methods must be used, or he might have added the old methods carried out to an unusual or exhaustive extent. Certainly, if a moderate application of the usual methods was sufficient for this decomposition, evidences of it would have been obtained long ago by the host of careful workers who have occupied themselves over these substances. Crookes has busied himself with the method of fractional precipitation (though not with special view to the testing of this question) and applied it most patiently and exhaustively to such substances as the rare earths, without obtaining results from which anything conclusive could be drawn. Victor Meyer seems to have believed that the decomposition could be effected by high temperatures and was very

hopeful of experiments which he had planned before his untimely death. Others have spasmodically given a little time to the problem but no one has thought highly enough of it to attack it with all of his energy.

Let us stop a moment and ask ourselves what would be attained if any one should succeed in decomposing an element by one of the usual methods. Has not this been done repeatedly in the past and merely served to add to the list of elements? Didymium has been made to yield praseo- and neodymium. That which was first called yttrium has been divided into erbium, terbium, and ytterbium and according to Crookes may possibly be still further decomposed. But these and similar decompositions are not generally accepted as offering any evidence that elements can be decomposed. It is merely the discovery of one or more new substances which have remained hidden in constant association with known bodies which were supposed to be simple. It would be necessary to prove that a single individual element had by the process adopted been actually decomposed and not some pre-existing impurity discovered. This, of course, would be exceedingly difficult and all such attempts as those mentioned can have little bearing upon the general question and can hold out slight hope of reward beyond the fame springing from the discovery of a new element.

Successful decomposition should mean much more. It should mean the discovery of a method which will decompose not one but many or indeed all of the elements and the decomposition of these must not yield a larger number of supposedly simple bodies but a small group of one or two or three which are common constituents of them all. It is quite idle to venture upon any prediction as to whether such a method will ever be discovered.

Setting aside, then, the direct experimental proof of the composite nature of the elements as unattainable at present, let us next examine the indirect evidence. It would seem wisest for the present to introduce under that heading the spectroscopic work of Lockyer. The results, while highly interesting, are too indefinite as yet to speak of as having a direct bearing. Yet a careful study of the spectra of the

elements leads us to a strong suspicion that the least plausible assumption is the one that the particles which give rise to such varied vibration are simple and unitary in nature.

Lockyer's most recent work, following up the line of his "Working Hypothesis" of twenty years ago, is very suggestive and may lead to important results. (Chemistry of the Hottest Stars, Roy. Soc. Proc. LXI, 148. On the Order of Appearance of Chemical Substances at different Temperatures, Chem. News 79, 145). Still too much must be assumed as yet for such work to be very conclusive. He writes of "proto-magnesium and proto-calcium" and Pickering discusses a "new hydrogen," all with an assurance and confidence which prove at least how deeply these changes in the spectra have impressed some of those who have most closely studied them.

But a more important method of indirectly testing the question is through a comparison of the properties of the atoms. Such a comparison has been made as to the atomic weights from almost the first appearance of a table of such weights. In other words, the idea of the composite nature of the elements followed very close upon the adoption of a stricter definition of them as simple bodies. Dalton, Prout, Döbereiner, Dumas, Cooke and many others have aided in developing the idea, some times faultily and harmfully, at other times helpfully. Some fell into the common error of going too far but all were struck by the fact that when these combining numbers, or atomic weights, were compared strange and interesting symmetries appeared. The times were not ripe for an explanation of their meaning and such crude assumptions as that of Prout that the elements were composed of hydrogen, or that of Low that they were made up of carbon and hydrogen, were too baseless to command much genuine support or to withstand much careful analysis. The important feature of agreement between such theories was the belief that the elements were composite and had one or more common constituents.

From the comparison of one property, the atomic weights, the next step was to the comparison of all the properties. This comparison is brought out clearest and best for us in the Periodic System. Here all the properties are very carefully

tabulated for us. The study of the system leads indisputably to the conviction that this is not an arbitrary but a natural arrangement, exceedingly simple in its groundwork but embodying most fascinating symmetries which hint of great underlying laws. He who looks upon it as a mere table of the atomic weights has lost its meaning. It tells with no uncertain note of the kinship of the elements and leads to a search after the secret of this interdependence and of their common factor or factors. There is so much which is made clearer if we assume a composite nature for the elements that many do not hesitate to make the assumption.

Still another indirect method of approaching the problem is by analogy with bodies whose nature and composition are known. A very striking symmetry is observed between the hydrocarbons, and these in the form of compound radicals show a strong resemblance to certain of the elements. This analogy need not be dwelt upon here. It has been recognized for a long time and tables of hydrocarbons have been constructed after the manner of the Periodic System. Now these bodies are simply built up of carbon and hydrogen in varying proportions and in any one homologous series the increments are regular. We know that they are composite and that they have but the two common factors, carbon and hydrogen.

Again the fact that certain groups of associated atoms behave as one element and closely resemble known elements may be taken as a clue to the nature of the elements. Thus carbon and nitrogen, in the form of cyanogen, behave very much like the halogens, and nitrogen and hydrogen, as ammonium, so closely resemble the group of elements known as the alkalies that this volatile alkali was classed with them before the era of our elements and the analogy lead to a vain search for an ' alkalizing principle' and later to an equally futile pursuit of the metal ammonium.

A further clue to this nature is afforded in the remarkable changes of properties which can be brought about in some elements by ordinary means and one might mention the equally remarkable veiling of properties induced by the combining of two or more atoms. Thus copper exists in a cuprous and a cupric condition and the change from one to the other can be

readily brought about. And this is true of many other elements.

This has doubtless been a tedious enumeration to you of well-known facts and arguments but it has been necessary, for I wish to lead you to the summing up of these arguments and to induce you to draw boldly the necessary deductions. It is high time for chemists to formulate their opinions in this matter. It would seem as if we were shut up to one or two conclusions. Either these imagined simple bodies are after all compounds built up of two or more common constituents or they are but varying forms of one, and the same kind of matter subjected to different influences and conditions. The supposition that they are distinct and unrelated simple bodies is of course a third alternative but to my mind this is no longer tenable.

The second hypothesis is the one put forth by Graham. It was his cherished vision of the gaseous particles about which he thought so deeply and in many ways so truly. Thorpe has written of this as follows (loc. cit. 222).

"He conceives that the various kinds of matter, now recognized as different elementary substances, may possess one and the same ultimate or atomic molecule existing in different conditions of movement. Graham traces the harmony of this hypothesis of the essential unity of matter with the equal action of gravity upon all bodies. He recognizes that the numerous and varying properties of the solid and liquid, no less than the few grand and simple features of the gas, may all be dependent upon atomic and molecular mobility. Let us imagine, he says, one kind of substance only to exist—ponderable matter; and further that matter is divisible into ultimate atoms, uniform in size and weight. We shall have one substance and a common atom. With the atom at rest the uniformity of matter would be perfect. But the atom possesses always more or less motion, due, it must be assumed, to a primordial impulse. This motion gives rise to volume. The more rapid the movement the greater the space occupied by the atom, somewhat as the orbit of a planet widens with the degree of projectile velocity. Matter is thus made to differ only in being lighter or denser matter. The specific motion of

an atom being inalienable, light matter is no longer convertible into heavy matter. In short, matter of different density forms different substances—different inconvertible elements, as they have been considered."

The hypothesis that the elements are built up of two or more common constituents has a larger number of supporters and would seem more plausible. Some have supposed one such primal element, by the condensation or polymerization of which the others were formed. Thus we have the hydrogen theory of Prout, modified to the one-half atom by Dumas and finally, by Zängerle, to the one-thousandth hydrogen atom. The suggestion of Crookes as to the genesis of the elements from a hypothetical *protyle* under the influence of electricity may also be mentioned here.

Others have adopted the supposition of two elements, Reynolds making one of these an element with a negative atomic weight, whatever that may mean. Low and others have fixed upon carbon and hydrogen as the two elements.

There are many practical difficulties in the way of these suppositions; the lack of uniformity in the differences between the atomic weight, the sudden change of electrochemical character and the impossibility so far of discovering any law underlying the graduation in the properties of the elements with the increase of atomic weights are some of the difficulties.

In comparing these two hypotheses, that of Graham seems to me very improbable. I have thought of valence as dependent upon the character of the motion of the atom but cannot well conceive of a similar dependence of atomic weight and all the other properties. There remains then the hypothesis of primal elements by the combination of which our elements have been formed. These molecules are probably distinguished from the ordinary molecules by the actual contact and absolute union of the component atoms without the intervention of ether.

Since these elemental molecules cannot as yet be divided we may retain the name atom for them but the idea of simplicity and homogeneity no longer belongs to them. The definition of an element as a body made up of similar atoms is equally lacking in fidelity to latest thought and belief but chemists would scarcely consent to change it, and indeed it may well be

retained provided the modified meaning is given to the word atom. But after all, an element is best defined by means of its properties. It is by a close study of these that we decide upon its elemental nature and through them it is tested. Complete reliance can no longer be placed upon the balance and the supposed atomic weight.

All elements are acted upon by chemical force and gravity and other physical forces, but within the last few years certain gaseous elements have been discovered which are not influenced by chemical force of affinity. According to some (Piccini Zeit. Anal. Chem. XIX, 295), this necessitates a division of the elements into two classes. Manifestly, since it is chiefly by the action of chemical force that we study the elements, the absence of such action cuts us off from our chief means of finding out anything about them and it is equally clear that bodies so diverse cannot well be classified together. If all attempts at bringing about the chemical union of these gaseous elements with other bodies fail I believe that we should insist upon the existence of two classes of elements and keep them distinct in all comparisons.

Of course, we are quite at a loss to say just what chemical force is but it is believed to be determined by the electrical condition of the atom. Thus we have the elements which show the action of chemical affinity varying from strongly electropositive to strongly negative. This electrical charge of the atom seems to be a primitive, inherent property and so beyond our control or power to change. At least no change of the kind has ever been recognized and recorded. Sodium remains positive and chlorin negative in spite of all that may be done to them. We can by uniting the two temporarily cloak and neutralize their opposite natures but the original condition returns on their release.

Is it not fair to assume that argon, helium and their companion gases, having no affinity are without electrical charge, atoms from which the electrical charge has been withdrawn, the deadest forms of inanimate matter? Were they thus without electrochemical properties and affinity from the beginning or did they start out as ordinary atoms (if I may so call them) and somehow, somewhere, lose these properties and with

them the power of entering into union of any kind, even of forming molecules, doomed to unending single existence? Can these be changed atoms of some of our well-known elements a step nearer to the primal elements and with the electrical charge lost? Is it possible for us to bring about these changes? May we not unwittingly have done so at some time or other in the past? Is it possible to restore the electrical charge to such atoms and so to place them once more on a footing of equality with elements of the conventional type? These and many other questions surge through the mind as one thinks of these wonderful gases. Perhaps the coming century will unfold the answer.

PAPERS READ.

[ABSTRACTS AND TITLES.]

FINAL REPORT OF THE COMMITTEE ON COAL ANALYSIS. BY W. A. NOYES, Chairman, Terre Haute, Ind.

THE RELATION OF PHYSICAL CHEMISTRY TO TECHNICAL CHEMISTRY. BY WILDER D. BANCROFT, Ithaca, N. Y.

ON THE CONSTITUTION OF OXYAZO COMPOUNDS. BY WILLIAM McPHERSON, Columbus, O.

The paper is an outline of the work I have been doing during the last three years on the oxyazo compounds, and which will be published in the American Chemical Journal shortly. The experiments definitely prove the constitution of the peroxyazo compounds and throw considerable light on that of the orthoxyazo compounds. The proof has been brought by the synthesis of a new class of compounds, by the action of unsymmetrical phenylhydrazines on peroxyquinones.

THE NATURE OF THE CHANGE FROM VIOLET TO GREEN IN SOLUTIONS OF CHROMIUM SALTS. BY W. R. WHITNEY, Jamestown, N. Y.

MICROSTRUCTURE OF ANTIMONY-TIN ALLOYS. BY J. J. KESSLER, JR.

THE ELECTROLYTIC DEPOSITION OF METALS FROM NON-AQUEOUS SOLUTIONS. BY LOUIS KAHLENBERG, Madison, Wis.

SOME EXPERIMENTAL ILLUSTRATIONS OF THE ELECTROLYTIC DISSOCI-
ATION THEORY. BY ARTHUR A. NOYES, Boston, Mass.

METHODS OF ANALYSIS OF SULPHITE SOLUTIONS USED IN PAPER-
MAKING. BY RUDOLPH DE ROODE.

IMPROVEMENT IN THE CHEMICAL COMPOSITION OF THE CORN KERNEL.
BY C. G. HOPKINS, Champaign, Ill.

SOME NEW PRODUCTS OF MAIZE STALKS, WITH ILLUSTRATIVE
EXPERIMENTS. BY H. W. WILEY AND W. H. KRUG.

The paper consists of an exhibition of samples of products made from
the maize stalk or parts thereof. The following samples together with
their chemical composition and a description of the methods of manu-
facture, are presented :

1. Compressed pith ; experiments showing its amount of increase in
volume on the addition of water.

2. Blocks of pith used for packing cofferdams of battle-ships.

3. Nitrated pith ; giving processes of nitration with an experiment
showing its burning qualities.

4. Various samples of smokeless powder made from nitrated pith ;
experiments showing burning qualities.

5. Paper pulp made from pith ; various grades and qualities.

6. Nitrated paper pulp, giving percentage of nitrogen and method of
nitration ; experiment showing burning qualities.

7. Pyroxylin varnish, made from paper pulp.

8. Smokeless powders made from paper pulp.

9. Samples of paper made from paper pulp.

10. Samples of nitrated paper made from paper pulp.

11. Cellulith made from the pith of the maize stalk with experiments
showing the qualities and use thereof.

HALIDES AND PERHALIDES OF THE PICOLINES. BY PAUL MURRILL.

SOIL HUMUS. BY E. F. LADD, Fargo, N. D.

The Relation of Fertilizers to Soil Moisture. By Julius T. Willard, Manhattan, Kans.

Secondary Heptylamine. By Thos. Clark, Chapel Hill, N. C.

Propane Trisulphonic Acid. By Wm. B. Shober, South Bethlehem.

On the Derivatives of Isuretin, of Formhydroxamic Acid, and Their Relation to Fulminic Acid. By H. C. Biddle, Chicago, Ill.

The paper gives an account of a new synthesis of fulminic acid, and of attempts to synthesize ethers of fulminic acid. The work was carried out by Mr. Biddle under the direction of J. U. Nef and was presented at the meeting by Mr. Nef.

The Reichert Figure of Butter. By Jas. H. Stebbins, Jr., New York.

The Determination of Nickel in Nickel-Steel. By George W. Sargent, Reading.

Camphor Acid, Alphahydroxydihydrociscampholytic Acid and the Synthesis of Dimethylcyancarbonethylcyclopentanone. By W. A. Noyes and J. W. Shepherd, Terre Haute.

The synthesis of camphoronic acid by W. H. Perkin, Jr., appears to exclude all formulae for camphonic acid except those of Perkin[1] (Bouveault[2]), Perkin[3] and Bredt.[4] If the first of these is correct the

1 Proc. Chem. Soc. (1896), 191.
2 Chem. Ztg., 21, 761 (1897).
3 J. Chem. Soc., 73, 798, 806 (1898).
4 Ber. d. chem. Ges., 26, 3047 (1893).

ketone described in the following paper must be 2-3-3, trimethylcyclo-pentanone. By condensing the ethyl ester of γ-brom-iso-caproic acid with sodium cyanacetic ester there is formed a dimethylcyancarboxethyl-cyclopentanone. Condensation products were also obtained with acetacetic ester and with methylmalonic ester. The yields are small but it is probable that its trimethylcyclopentanone which is desired can be obtained from one of these compounds.

CAMPHONIC ACID: α-HYDROXYDIHYDROCISCAMPHOLYTIC ACID. BY WILLIAM A. NOYES AND J. W. SHEPHERD.

By shaking the ethyl ester of α-bromdihydrociscampholytic acid with a solution of barium hydroxide at 30°-40° for some days about ten per cent. of the ester is converted into α-hydroxydihydrociscampholytic acid. The acid is difficultly soluble in water and melts at 111°. On boiling the acid with lead peroxide and dilute sulphuric acid a ketone, $C_8H_{14}O$, is formed. It boils at 167°-169° and has a specific gravity of 0.8956 $^{2}\text{\textfrac{0}{0}}$. The oxime melts at 104°. The ketone condenses with only one molecule of benzaldehyde giving a compound, $C_8H_{12}O = CHC_6H_5$, which melts at 74°. This demonstrates that the ketone contains but one methylene group adjacent to its carbonyl. This excludes the formula for camphonic acid recently advocated by Perkin.[1]

The full account of this work will appear in the American Chemical Journal, and a short account in the *Berichte*.

DIAZO-CAFFEINE. BY M. GOMBERG, Ann Arbor, Mich.

THE PREPARATION OF TRIPHENYLCHLORMETHANE AND TRIPHENYL-CARBINOL. BY M. GOMBERG, Ann Arbor, Mich.

THE ACTION OF SODIUM METHYLATE UPON THE DIBROMIDES OF PRO-PENYL COMPOUNDS AND UNSATURATED KETONES. BY. F. J. POND.

Paper transferred to Section "D."

1 J. Chem. Soc. **73**, 798, 806 (1898).

SOME SECONDARY CYCLIC AMINES. BY CURTIS C. HOWARD, Columbus, Ohio.

ON NAPHTHALENEAZOALPHANAPHTHOL AND ITS DERIVATIVES. BY WM. MCPHERSON AND ROBERT FISCHER.

The formation of this by the action of naphthyl hydrazine on a naphthoquinone, was studied. A number of its derivatives were also prepared and their properties studied.

THE IODOMETRIC DETERMINATION BY SMALL OF CARBON MONOXIDE. BY L. P. KINNICUTT AND GEORGE R. SANFORD.

ESTERIFICATION EXPERIMENTS WITH HEXAHYDRO- AND TETRAHYDROXYLIC ACIDS. BY WM. A. NOYES.

The neighboring xylic acid, C_6H_3
$$\begin{matrix} CH_3 & 1. \\ CO_2H & 2, \\ CH_3 & 3. \end{matrix}$$
obeys Victor Meyer's law of esterification, less than one per cent. of it being converted into the methyl ester by boiling with methyl alcohol containing hydrochloric acid for several hours. Under similar conditions about one-third of the hexahydro or tetrahydroxylic acids are converted into the corresponding esters. A similar experiment will be made, if possible, with dihydroxylic acid. If this is found to correspond to the xylic acid in esterification, a strong support will be given to Kekulé's formula for benzene.

ON THE CONDENSATION OF CHLORAL WITH ORTHO-, META-, AND PARA-NITRANILINES. BY CHARLES BASKERVILLE, Chapel Hill, N. C.

NOTES ON THE OCCURRENCE OF CHROMIUM, TITANIUM, AND VANADIUM IN PEATS. BY CHARLES BASKERVILLE, Chapel Hill, N. C.

ON THE UNIVERSAL DISTRIBUTION OF TITANIUM. BY CHARLES BASKERVILLE, Chapel Hill, N. C.

The Atomic Weight of Calcium.   By T. W. Richards.

Preliminary Report on a New Method for the Determination
  of Carbon Dioxide.   By M. E. Hiltner.

Examination of Flavoring Extracts.   By A. S. Mitchell.

The Composition of American and Foreign Dairy Salt.   By F.
  W. Woll, Madison, Wis.

In all, eighty-one samples of dairy salt representing the main brands at
present on the market in this and foreign countries, were subjected to
chemical and mechanical analysis.   The results show that there are but
small differences between the best American brands of salt as regards
their chemical composition or their value for butter- and cheese-making,
and that all of the best domestic brands are equal to or, in some cases,
even superior to the best foreign dairy salts, so far as can be determined
by the chemical composition of the salts or their mechanical composition.

(This paper will be printed as bulletin No. 74, of Wisconsin Agricultural
Experiment Station.)

Notes on Testing Soils for Application of Commercial Fertili-
  zers.   By H. A. Weber, Columbus, O.

The method of testing soils as described in the paper was devised by
the author in 1885.

The paper treats of the manner of collecting an average sample of soil
of about seventy-five pounds ; of the necessity of the soil being in a high
state of cultivation ; of the useless expenditure of money by applying
commercial fertilizers to soils already rich enough in plant food ; of a ra-
tional application of fertilizers by ascertaining what the soil needs ; of the
method of conducting the experiments, and of observations to be made
during the growth of the plants.

A Determination of the Transformation Point of Sodium Sul-
  phate.   By A. P. Saunders, Ottawa, Canada.

The sharpness and constancy of the point of temperature where sodium
sulphate loses its water of crystallization have suggested the use of a bath

of this salt for testing thermometers and for other similar purposes. The determination of this point by Richards yielded results which do not agree either with those of Loewenberg or with those of Meyerhoffer and the author. It seemed desirable therefore to make as accurate a determination as possible, using an air thermometer. To that end a special form of thermometer was devised, the type being the sulphuric acid air thermometer as used by Callendar; the instrument was however supplied with a ⨆-tube in the capillary arm; this ⨆ contained sulphuric acid standing higher in the outer than in the inner arm. In this way it was possible to plunge the bulb and stem so far into the bath that only the outer arm of sulphuric acid was visible and by bringing this to a fixed mark in each determination, all correction for temperature and volume of the arm was eliminated. The results obtained gave a mean value of 32.44° C. for the transformation point. The paper contains also a brief discussion of the value of a quintuple point; namely, the transformation point of sodium sulphate in the presence of an excess of sodium chloride, for obtaining a constant temperature bath.

NOTES ON ESTIMATION OF TOTAL CARBON IN IRON AND STEEL. BY F. P. DUNNINGTON, Charlottesville, Va.

ELECTROLYSIS OF METALLIC PHOSPHATE SOLUTIONS. BY HARRY M. FERNBERGER AND EDGAR F. SMITH.

THE ACTION OF SODIUM METHYLATE UPON THE DIBROMIDES OF PROPENYL COMPOUNDS AND UNSATURATED KETONES. BY F. J. POND, O. P. MAXWELL, AND G. M. NORMAN.

ON THE DETERMINATION OF VOLATILE COMBUSTIBLE MATTER IN COKE AND ANTHRACITE COAL. BY RICHARD K. MEADE AND JAMES C. ATTIX.

OBSERVATIONS ON TUNGSTEN. BY EDGAR F. SMITH, Philadelphia, Pa.

THE ATOMIC MASS OF TUNGSTEN.   BY WILLETT L. HARDIN.

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NOTES ON THE DETERMINATION OF SULPHUR IN PIG IRON.   BY M. J. MOORE.

———————

THE CHEMISTRY OF RANCIDITY IN BUTTER-FAT.   BY C. A. BROWNE, JR.

———————

AN ELECTROLYTIC STUDY OF BENZOIN AND BENZIL.   BY JOSEPH H. JAMES.

———————

THE QUANTITATIVE ESTIMATION OF BORIC ACID IN TOURMALINE.   BY GEORGE W. SARGENT.

———————

SOME BOILING-POINT CURVES.   BY J. K. HAYWOOD.

———————

ELECTROLYTIC DETERMINATIONS AND SEPARATIONS.   BY LILY G. KOLLOCK.

———————

THE PRECIPITATION OF COPPER BY ZINC.   BY JOHN G. SHENGLE AND EDGAR F. SMITH.

———————

DERIVATIVES AND ATOMIC MASS OF PALLADIUM.   BY W. L. HARDIN.

———————

ACTION OF HYDROCHLORIC ACID GAS UPON SULPHATES, SELENATES, TELLURATES, AND PHOSPHATES.   BY RAYMOND W. TUNNELL AND EDGAR F. SMITH.

THE ELECTROLYTIC OXIDATION OF SUCCINIC ACID. BY CHARLES H. CLARKE AND EDGAR F. SMITH.

THE PERSULPHATES OF RUBIDIUM, CÆSIUM, AND THALLIUM. BY ARNOTT R. FOSTER AND EDGAR F. SMITH.

THE CHEMICAL COMPOSITION OF BUTTER-FAT. BY C. A. BROWNE, JR.

THE ELECTRICAL CONDUCTIVITY OF LIQUID AMMONIA SOLUTIONS. BY E. C. FRANKLIN AND CHARLES A. KRAUS.

In a recent paper,[1] the authors called attention to some properties of liquid ammonia, among which is the remarkable power exhibited by this liquid as an electrolytic solvent.[2]

In the present paper is given a description of the apparatus and method used in making measurements of the conductivity of substances in solution in liquid ammonia, together with the results obtained.

Considerable difficulty was experienced in obtaining the pure solvent, but after the method of purification had been perfected, ammonia was obtained having a specific conductivity lower than 0.01×10^{-6} (0.01×10^{-10}).

The method of Kohlrausch with bridge and telephone was used.

The molecular conductivity of the following compounds in solution in liquid ammonia have been measured. The measurements were made at the boiling-point of ammonia at atmospheric pressure ($-38°$). The new units of Kohlrausch[3] are used. Approximate values of Δ are given.

[1] Am. Chem. J., Jan., 1899.
[2] Cady : J. Phys. Chem., 1, 707.
[3] Kohlrausch, Holborn, and Diesselhorst : Wied. Ann., 64, 417 (1898). Kohlrausch and Holborn : Das Liebvermögen der Electrolyte, p. 1, et seq.

$\Phi \times 10^{-8}$ (v.)	50	100	1,000	10,000	50,000
Potassium bromide	—	—	262	326	339
" iodide					
" nitrate	—	—	245	320	335
" metanitrobenzene-sulphonate	—	—	294	259	281
Sodium bromate	—	—	217	272	282
" bromide	—	—	243	290	300
" nitrate					
Ammonium chloride	—	—	212	289	303
" bromide	—	—	—	290	300
" nitrate	—	161	247	292	297
Silver cyanide	20.2	20.8	21.5	—	—
" iodide	—	—	124	225	272
" nitrate	—	—	248	288	—
Mercuric cyanide	1.20	1.20	—	—	—
Strontium nitrate	—	—	219	335	471
BASIC AMIDES.					
Sodamide	6.5	10.1	33	—	—
ACID AMIDES.					
Acetamide	0.5	0.6	—	—	—
Benzenesulphonamide	20	47	67	190	—
Metanitrobenzenesulphon-amide	—	92	163	216	235
Orthomethoxybenzenesul-phonamide	—	14	38	95	149
Metamethoxybenzenesul-phonamide	22	29	73	153	—
Paramethoxybenzenesul-phonamide	13	16	47	106	—
Saccharine	—	83	139	194	—
NITROCOMPOUNDS.					
Nitromethane	16	22	57	145	—
Metadinitrobenzene	—	—	168	225	—
Trinitrotoluene	—	—	190	223	—
Orthonitrophenol	—	—	190	223	—
Benzaldehyde	2.5	3.2	—	—	—
Metallic sodium	400				

The limit of molecular conductivity of binary salts is reached at dilutions between $\Phi 10^{-8} = 50,000$ and $\Phi 10^{-8} = 100,000°$. The values lie between $\Delta = 270$ and $\Delta = 340$, and are more than twice greater than aqueous solutions of the same dilution at 18°.

The halogen salts of silver are good conductors of electricity; but one of them, however, the iodide, has been measured quantitatively.

Mercuric cyanide has about six times the conductivity in ammonia solution at —38° that it has in aqueous solution at 18°. The molecular conductivity decreases with the dilution.

Acid and Basic Amides.—Sodamide and such acid amides as have yet been studied are good conductors of electricity. It is pointed out that these two classes of compounds bear a relation to ammonia similar to that borne by the ordinary oxygen acids and bases to water.

In ammonia solution the following reactions have been studied qualitatively :

(1) $NH_3 + Na = NaNH_2 + H$

(2) $C_6H_5SO_2NH_2 + NaNH_2 = C_6H_5SO_2NHNa + NH_3$

(3) $C_6H_5SO_2NH_2 + CuO = C_6H_5SO_2NHCu\frac{1}{2} + \frac{1}{2}H_2O$

(4) $C_6H_5SO_2NH_2 + Mg = C_6H_5SO_2NHMg\frac{1}{2} + H$

(5) $C_6H_4\genfrac{}{}{0pt}{}{CO}{SO_2}{>}NH + Zn = C_6H_4\genfrac{}{}{0pt}{}{CO}{SO_2}{>}NZn\frac{1}{2} + H.$
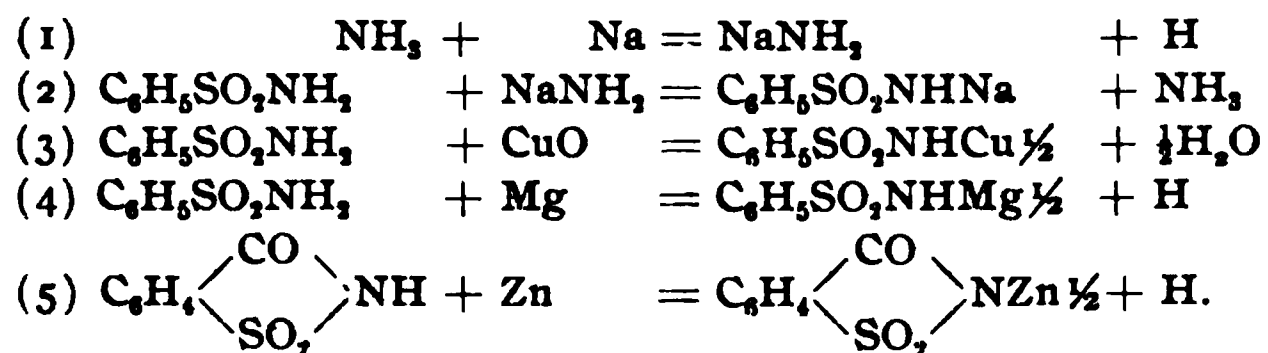
The nitro-compounds, some of which dissolve to brilliantly colored solutions, are good conductors of electricity.

Benzaldehyde is the only compound of this class which has been measured, other aldehydes tested qualitatively showed themselves to be conductors.

The alkali metals in solution in ammonia are remarkably good conductors of electricity. The molecular conductivity seems to increase somewhat with dilution. These solutions have positive temperature coefficients.

SECTION D.

MECHANICAL SCIENCE AND ENGINEERING.

II

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Member of General Committee.
W. T. MAGRUDER, Columbus, O.

ADDRESS

BY

STORM BULL

VICE-PRESIDENT, AND CHAIRMAN OF SECTION D.

ENGINEERING EDUCATION AS A PRELIMINARY TRAINING FOR SCIENTIFIC RESEARCH WORK.

At first thought it would perhaps seem that the subject chosen for this address is of such a nature that it should rather have been made the basis of a paper, which ought to have been presented at the meeting of the Society for the Promotion of Engineering Education. I admit that it would not have been out of place there, but at the same time I am also of the opinion that such an address also forms, as it were, a bridge from our special engineering section to the general Association. It will show that the work and the attainments of the engineer form an important and integral part of the scientific work of to-day.

As you no doubt know, there has been for some time general and strong misgivings as to the future of this section of the Association, and a good many have expressed the opinion that engineers and professors of engineering ought not to belong to the A. A. A. S., as the work of the engineer and of the pure scientist were of such a very different nature. It must, of course, be granted that the work of most practicing engineers is only distantly related to the work of the members of this Association belonging to the various sections, with the exception of D. But on the other hand a great many of the practicing engineers and of the professors of engineering do

truly scientific work, and what is more, in the opinion of the speaker, the preliminary training of the engineer is perhaps the best one yet found to educate a man for future scientific research work.

These considerations have led the speaker to believe that a consideration of the subject announced might perhaps increase the interest in section D, and possibly thereby help to prevent its disappearance, which to many of us has seemed both imminent and deplorable.

Presumably our friends, the pure scientists, will shake their heads significantly when they read the title of this address and if any of them should happen to hear it, or later read it, they might perhaps even go so far as to bestow a smile of pity on us poor engineers, etc., who have such a high opinion of their own worth. But even if none of our scientific brethern should be converted, yet the speaker should feel satisfied with the results, if he should have given more confidence to the members of the engineering profession in its broad sense as possessing the necessary training for accurate and important scientific research work.

The proposition which I expect to defend in this address is that the engineering education as furnished in the best technical schools of the world, together with the training obtained later in life as a practicing engineer, furnishes probably the best preliminary preparation for the successful prosecution of scientific research work. I am now speaking of the preliminary training ; the special knowledge of the subject in which the research work is to be done must of necessity be acquired on top of this engineering education, except when such work is to be done in some one of the engineering branches. I desire also to call your attention to the fact that I do not mean to say that it is the only true method to gain the desired end, but that I have the feeling, that although all roads lead to Rome, yet this one is perhaps the most direct one, possibly also the best paved one.

The subject of scientific research work is, as I understand it, to ascertain the facts of nature, to correlate these facts, and finally to deduce the laws of nature as illustrated by the facts discovered. It is probable that a better definition might be

given, but for the purpose of this address it is desirable that the various objects of scientific research work be given in something like the form above, and I feel very confident that the definition is one against which no serious objection can be raised. It will be noticed that I divide the scientific research work into three parts, and I am sure that everybody will agree, when I say, that the most of the scientific work done to-day is done along the first line. To a less extent the work of correlating the facts discovered is done, and the work of drawing the conclusions from the facts thus correlated in establishing the laws of nature, is work of such a nature that but few of the scientific workers get so far. That this is the case is but natural, because of the difficulties of the problem, and although it is the most important of all scientific research work, yet, even of those who work in this special line, there are but few who are able to draw the correct conclusions, and this because of the lack of proper judgment in weighing the importance of supposed evidence and facts, or because of lack of previous training in suppressing the natural tendency to overestimate the value of one's own labor. I take it for granted that everybody appreciates the difficulties and failings, which because of the previous preparation, or perhaps better because of the lack of previous preparation, necessarily attaches to the scientific research work of to-day. An inquiry into the necessary and desirable qualifications for a man working in this line, will, I think, most rapidly lead us to the heart of the question raised by this address. Is not the engineering education a remedy for a good many of the weaknesses found in the ordinary scientific man?

These qualifications will here be given separately as affecting the three kinds of work into which I have divided all scientific research work. First, what should be the previous training of a man who is to ascertain the facts of nature. He must necessarily have his faculties of observation trained to the highest degree, so that he sees the facts as they actually are, and perhaps more important yet, that he can see the single fact, which he is looking for, without being disturbed by the surrounding, which in the eyes of the untrained man would obscure the perhaps small object for which the investigator is

looking. It is also very necessary that the observer should be able to accurately describe the object, or fact seen. His mental habit should be such that accuracy is a necessity. And finally I think that the scientific worker should for many kinds of observations have his hands trained in such a manner that necessary apparatus can be used intelligently and even designed and made.

Second, what are the special qualifications for collecting and correlating the isolated facts of nature, as established by others? It seems to me that if anybody is to do this successfully, he must possess all the qualifications of the worker in the first line, except that possibly he does not need the manual dexterity which was necessary for the original investigator. In order to properly classify the facts according to already existing rules and laws, he must also be familiar with the methods along which the facts have been established, in order that he shall be able to judge whether the results obtained by the original investigators are really facts, or only delusions. This man must therefore necessarily have a wider experience and outlook than the first observer, and he must possess a sharper judgment which can only be obtained by special training.

Third, the scientific man, who from a large amount of material, collected and correlated by others, shall be able to draw correct solutions so as to establish the laws of nature along which nature works, must necessarily have very special qualifications. He must not only have the wide outlook, a profound learning in his special branch, he must in addition be sober minded, must be able to weigh evidence as thoroughly and impartially as the best qualified judge, and must not alone see clearly, but be able to go behind the returns so as to be in a position to decide whether the evidence presented is a proper one in the case in question, or even if it be the evidence really represents facts. Having sifted the evidence he must be capable of so overlooking the field that the general law of which the isolated facts are exponents, will reveal itself to his mind. For this latter purpose and to prevent visionary conclusions, I take it, that a rigid training in accuracy and sobriety is required.

It is my contention that a man who has received a thorough engineering education, and perhaps has added a few years of professional work to the scholastic training, is as well prepared to take up scientific research work as anybody coming from our universities and colleges. I do not think that anybody will deny that the work which is required of the engineering student in our best colleges tends very largely to establish a habit of accuracy, which, as was pointed out before, is one of the most essential qualifications of a scientific man. There is no study like mathematics, with its various applications, to teach a man accuracy, and as this study forms the backbone in all engineering courses of study, it is only to be expected that the engineering student, when he leaves college, shall have gotten into a habit of mind which makes it impossible for him to be inaccurate, either in his work, or in his mode of expression. It is also one of my notions that the study of mathematics teaches truthfulness and sobriety of thought. As was explained before, the latter I deem one of the most essential qualifications for the man who is to do the highest grade of scientific work. The engineering education and the practice of the engineering profession necessarily will teach this sobriety of thinking more thoroughly than any other kind of education. The object of the engineering professions is to utilize the laws and forces of nature for the wellbeing of the human race. Consequently the engineer must build on the laws of nature, must apply them, and the results of such application we see in the innumerable achievements of the engineer of modern times. The true engineer first surveys the field, then makes his plans and computations based on his observations and the laws of nature, and the result of such work is, for instance, either the machine which is to do a certain work, or the bridge which is to carry the modern heavy railroad train, or it may be one of these monster buildings which in the last few years have been erected in the large cities. If the preliminary work of the engineer has not been accurate, or if he has not applied the laws of nature correctly, the result is inevitable ; the machine will not do its work, the bridge will not carry the train, or the tall edifice building will not carry the enormous weight concentrated in it. The punish-

ment will follow the mistake of the engineer, as surely as the earth keeps on moving around the sun. This is the great point in the engineering education, which, at any rate in some respects, makes it the best preliminary training for men who are to do scientific research work. A good engineer is necessarily an accurate man, he is necessarily also a soberly thinking man, and thirdly he must also possess a discriminating judgment, as the results which follow superficial reasoning or visionary planning are fatal to all engineering work. There are no studies which teach this lesson as strongly as the various professional engineering studies, and it seems to me, therefore, that one of the most essential qualifications for doing thorough scientific work is obtained in a higher degree by the engineering education than by any other training. It is true that the ordinary engineering student has but limited opportunity to test his plans and computations in the actual execution of the plans. But it must be remembered that the student is always reminded of the inevitable results of even one false step—both in the classroom, laboratory and draughting room, and that his work is controlled by men who are supposed to have had the necessary experience in practical life. That the actual practice of the engineering profession is the best teacher in this line needs hardly be stated. The work of the engineering student in the draughting room, in the shop and in the laboratory, fits him peculiarly for scientific research work, as he there gains the dexterity of his hands, while his facilities of observation are being trained, and he learns to be accurate and neat. Another fact which, it seems to me, speaks in favor of the engineering education, is that the course as laid out in all of the best engineering colleges requires very hard and persistent work on the part of the student in order that he shall be able to keep up with the requirements, which in all cases are higher than in the other courses at our universities. This preliminary training as given by the engineering course would therefore teach the future scientist the habit of hard work, which of necessity is of great importance. What is still more the engineering courses comprise principally such studies that require clear and hard thinking, and the engineering student, when leaving college, should have acquired the habit

of clear thinking without which scientific research work is not of any value.

The only objection which perhaps might be raised against my contention, is that the engineering courses of study are narrow in their nature, and that the graduate of an engineering college consequently will be a narrow man. It is true that in a certain sense the course of study is narrow, it does not include any classics for instance, nor does it include as much of the humanities as is desirable. But on the other hand specialization has gone so far in the present day, that I think I am correct when I state, that, for instance, the ordinary classical course, with its excessive amount of Greek and Latin is fully as narrow as the engineering course ; and, as to the scientific college course it is enough to say, there is no reason why it should be deemed less narrow than the engineering course, except for the fact that specialization has not been carried so far in the first as in the latter. The ideal engineering education is first an academic course, followed by two or three years' work in the engineering college, and if such length of time of study is not deemed too much for the profession of a lawyer, as actually is the case, there is no reason why it should be too long for the engineer. A man educated as just indicated would certainly be better fitted for scientific research work than any other college graduate who had an equal amount of time for his preparation, but had taken no engineering work.

That the engineer of the present day is doing a large amount of scientific research work does not need any proof, and because of his training I am of opinion that his work is of a better quality than that of the ordinary scientific man. More reliance can be placed on it as it necessarily has had to undergo a more severe test, both for accuracy and soundness in conclusion, than if it had been done by a person who had not had the preliminary training of an engineer.

To disprove this statement I suppose that some one very likely might mention the name of Kreidler, or perhaps even that of Keely, but it is sufficient to state that these men are not, nor were they ever, engineers, and it might also be pointed out, that the engineers are not responsible for any of the perpetual motions, which even in this enlightened day, seem to be as numerous as they ever were.

In conclusion I desire to repeat that we engineers, or semi-engineers, need to feel that our work is very often scientific research work of the highest character, and that although we are very often told that because of its practical nature, it does not belong to pure science, yet we should insist that whether it be pure science or not,—yet it is scientific work—and because of our previous training it is likely to be of permanent value.

I desire to offer an apology for the shortcomings, probably altogether too visible in this address, and to express the hope that section D of the A. A. A. S., because of the large and important field which it represents, will take a new start and enter upon a new era of prosperity.

PAPERS READ.

[TITLES AND ABSTRACTS.]

SUPPORTS FOR BEAMS IN TESTS OF TRANSVERSE STRENGTH. BY PROF. WM. T. MAGRUDER, M.E., Ohio State University, Columbus, Ohio.

The paper was illustrated by photographs and the supports themselves. The latter consisted of (*a*) fixed and rigid supports with and without ball bearings, (*b*) flexible supports made of saw plate steel to which were attached angles so as to form "channels" or Z bar sections, (*c*) flexible supports resting on ball bearings in cradles swinging freely about axes normal to the beam, which may be made to pass through the lowest, the center, the upper or any other desired fiber of the beam. The author stated that the usual supports as furnished with testing machines were found to be unsatisfactory and the above were designed and have successfully overcome all objections.

ELECTRIC MINING OF BITUMINOUS COAL. BY PROF. W. S. ALDRICH, University of Illinois, Champaign, Ill.

THE ILLUSTRATION OF CRITICAL SPEEDS OF SHAFTS. BY PROF. THOMAS GRAY, Rose Polytechnic Institute, Terre Haute, Ind.

This paper referred to the difficulty sometimes encountered in obtaining satisfactory balancing of the rotatory parts of machinery when the speeds are high. The difficulty is partly due to change of shape of the parts during the rotation but an important factor, which seems to be too little attended to in the discussion of machine design, is synchronism of the free period of vibration with that of its rotation. The free period of vibration can in many cases be foretold with a fair degree of accuracy and hence by preliminary calculation and a proper adjustment of the sizes of the parts, all danger from this cause may be avoided. A simple apparatus used by the author for illustrating the importance of this subject was described. The apparatus consists of a shaft of convenient size and length mounted on ball bearings at its ends and provided with a load in the form of a solid disk at its center. The disk is provided with a pair of balan-

cing screws which can be screwed closer or further from the axis of rotation along diameters of the disk at right angles to each other. The shaft and disk are driven through any convenient flexible connection by means of a cöaxial shaft fitted with a driving disk which can be moved along the shaft while it is in motion and the edge of which bears against the face plate of the lathe. By means of the speed cones of the lathe and the backward or forward motion of the driving disk the speed may be varied through a wide range. It is convenient to have several load disks of different mass, any one of which can be fitted to the shaft so as to enable the effect of the mass of the rotating parts to be illustrated. It is found that the speed of the shaft may be varied through a wide range giving a steady motion with comparatively rough balancing. If, however, one particular speed be reached violent vibrations are quickly set up. For this speed it is found almost impossible to so adjust the balancing weights that the vibration will not occur and become of dangerous amplitude. A simple measurement of the deflection of the shaft produced by the weight of the disk and a comparison of the consequent free period of vibration with the period of rotation will show them to be the same. Some vibration usually occurs when the period of rotation is half that of vibration but this is generally unimportant. With the apparatus used by the author the critical speed, with the disk commonly used, is 840 revolutions per minute. When the balance is imperfect a moderate vibration gradually sets up at 420 revolutions. The shaft may be run with the steadiness of a spinning top at 800 or at 900 and up as high as 1700 the highest to which it has been subjected.

A simple attachment is used for drawing a diagram of the amplitude of vibration at different speeds but such is not required for purely illustrative purposes.

A NOVEL METHOD OF TESTING A LOCOMOTIVE BOILER. BY PROF. FRANK C. WAGNER, Rose Polytechnic Institute, Terre Haute, Ind.

CRYSTALLIZATION IN BRONZE TEST PIECES. BY PROF. WM. T. MAGRUDER, Ohio State University, Columbus, Ohio.

The paper described experiences and observations noted in testing bronze. The broken specimens and photo-enlargements of the ground etched surfaces were shown and attention was called to the shape of the contracted section and the arrangement of the crystals.

THE FRICTION OF BALLS IN THRUST BEARINGS. BY PROF. THOMAS GRAY, Rose Polytechnic Institute, Terre Haute, Ind.

During the past winter a number of experiments have been made in the

Engineering Laboratory of the Rose Polytechnic Institute with the view of determining the friction of balls when used in thrust bearings. The results obtained have not been very satisfactory owing to the great difficulty experienced in obtaining surfaces of uniform hardness. The method employed in making the measurements was, however, found to answer the purpose very well and the paper referred mainly to that.

The apparatus employed consisted of three concentric disks mounted on a spindle in such a way that the two outside disks were turned when the spindle turned but were free to move from or towards each other. The middle disk was an easy fit on a ball race groove turned in the spindle. The balls were placed between the center disk and the two outside ones and pressure was applied by putting the spindle in a drill press and the disks between a shoulder on the spindle and a horizontal plate which was free to turn on a ball race beneath it. The supporting plate rested through its ball bearings on the platform of a steelyard by means of which the pressure was measured. The central disk was prevented from turning by means of a spring dynamometer applied to it which also served to measure the turning moment required to keep it at rest. In order to cause all the balls to move on concentric paths of nearly the same radius, the faces on which the balls rolled were made slightly conical, the upper surface of each pair being slightly more conical than the lower. This caused the distance between the disks to be slightly greater at the outer circumference than at the spindle. Any slight difference of diameter of the ball was in this way automatically allowed for. No constraining ring was placed round the balls which rolled freely on two point bearings.

When a number of balls are placed between two plates so as to lie on a ring concentric with the center of rotation and relative rotation set up between the plates, the balls gradually increase the diameters of their paths, those having the greater pressure applied to them moving out the faster. Bearing that in mind it will be seen that the arrangement adopted allowed the balls, not only to find the paths corresponding to their diameter, but also caused them to take equal shares of the applied pressure.

The fact that the diameter of the path gradually increases was taken advantage of to include in each experiment the effect of diameter of path on the turning moment required to overcome the friction. Each experiment consisted in placing a ring of balls round the spindle on each side of the central plate, applying a definite pressure, starting up the drill press and observing the dynamometer reading corresponding to a number of ball race radii. On account of the difference in steepness of the upper and lower conical surfaces on which the balls rolled, the drill press spindle had to be fed down as the balls increased the diameter of their path. The amount of this feed was used to indicate the average diameter of the ball race.

The surfaces on which the balls rolled were of hardened steel, ground true and polished after hardening.

The results obtained indicate that so long as the pressure is not great

enough to produce permanent set on the race, the turning movement anc be expressed by an equation of the form

$$M = ap\mathbf{i} + bpr$$

where p is the pressure on each ball, r the radius of the race, and a and b constants.

For example : With high-grade tool steel such as is used for milling cutters and so forth and balls $\frac{1}{2}$ inch in diameter the constants were found to be, in inch pound units;

$$a = 0.0043$$
$$b = 0.0032$$

The actual moment for the double ring of balls as used in the experiment was double that obtained by using these constants.

It would appear from these results that the turning moment is made up of a constant pivot friction, such as would be obtained if only one ball were used and placed at the center of rotation, together with a constant rolling friction proportional to the pressure, giving greater and greater moment as the radius increases.

SOME ENGINEERING EXPERIENCES WITH SPANISH WRECKS. BY PROF. WM. S. ALDRICH,[1] Professor of Electrical Engineering, University of Illinois.

Before the late Spanish-American war, the Engineer-in-chief of the Navy, Rear Admiral Geo. W. Melville, U. S. N., brought to the attention of the Secretary of the Navy, " the desirability of making such preparation for the fitting out of a vessel which would be a floating repair shop as would enable the work to be done with great rapidity when needed." The Steamship Chatham was purchased for this purpose by the Government, overhauled and fitted out at the Boston Navy Yard in about six weeks, renamed the Vulcan, reported to Admiral Sampson, July 1st, 1898, and came to anchor at the naval base in Guantanamo Bay, Cuba.

Skilled mechanics and helpers were enlisted to serve for one year. There were 89 of the former, 20 of the latter, with 39 men on the engineers' detail and 70 men in the ship's crew, making 218 in all. These men were fortunately of the most varied attainments outside of their trades, adapting them to a wide range of most helpful service in executing the Vulcan's program. Few knew their own resourcefulness till put to the test, at sea, in a tropical climate, more than a thousand miles from their typical New England workshops.

The Vulcan was fitted out with the most approved workshop appliances for iron and brass casting, forging, brazing, boiler-making, pattern-making, and machine work required in making all kinds of naval repair

[1] Late Passed Assistant Engineer, U. S. N., attached to the U. S. Naval Repair Ship Vulcan.

in foreign waters. This equipment was all placed on the " shop deck," within about two feet of the load water line, making it very accessible for receiving and delivering repair work through the four large cargo ports on this deck. The 36-inch cupola for melting iron, of capacity of 3000 lbs. at one heat, was placed on the cement foundry floor, sixteen feet square, which contained also the two brass furnaces. Each of these had separate stacks, extending above the top sides, about thirty-three feet from the foundry floor. The power-bending rolls for boiler and plate work, would take in a plate six feet wide and a half-inch thick. In the machine shop were 30-inch and 36-inch lathes, 36-inch by 36-inch by 6-foot planer, 24-inch sharper, 22-inch milling machine, and a 3-foot radial drill, besides many smaller lathes, sharpers, drill presses, milling machines, grinders, and small tools of every variety.

The large and varied assortment of engineers' and ships' stores enabled us to fill many requisitions at once or to manufacture the needed supplies from rough stock. The operation of a foundry at sea, the manufacturing of many lines of naval supplies, the execution of repairs for all departments of naval vessels, equipment, navigation, ordnance and construction beside that for their steam motive-power machinery, show conclusively that this " engineers' shop" was in no sense an experiment.

In the first forty-one days of the Vulcan's work in Guantanamo Bay, there were 528 orders for repairs filled and 256 requisitions for supplies. Capt. E. F. Chadwick, U. S. N., Commanding Officer of Admiral Sampson's Flagship, the U. S. S. New York referring to the work of the Vulcan in maintaining the efficiency of the fleet in Cuban waters, reported : " Thoroughly efficient and well looked-after machinery mean facilities for instant repair. No one can understand the value of such an adjunct who has not had to look around for ships to go on duty."

After the battle of July 3, all of Admiral Sampson's fleet came to Guantanamo Bay for overhauling, repairs, and supplies. The next movement was here rapidly prepared for by the Vulcan's work on the vessels of Admiral Watson's Eastern Squadron for the coast of Spain. These vessels were finally reported ready to set sail, when the Peace Protocol was signed, Aug. 12, 1898.

A few weeks later the 100-ton Spanish gunboat Sandoval was raised by the U. S. S. Potomac, brought alongside the Vulcan, overhauled, repaired, and fitted out ready for sea. The Spanish armored cruiser Infanta Maria Teresa, was successfully floated off the beach, Sep. 23, and towed to Guantanamo Bay. After about five weeks of uninterrupted work, by the larger part of the Vulcan's force the Teresa was ready for sea, a volunteer crew was called for and 44 men selected with 70 Cuban helpers. Under her own steam, in the starboard engines and in tow of the Vulcan and the wrecking tug Merritt, the Maria Teresa started north, bound for the Norfolk Navy Yard, in convoy of the Leonidas.

In the early morning of Nov. 1, a violent hurricane struck the fleet. It was impossible to keep the Maria Teresa head to wind, she rolled and pitched in the trough of the sea, shipping large quantities of water at

every list. Her pumps became choked, engine- and boiler-rooms flooded, and the whole motive power of little avail. In this extremity it was decided to abandon ship and save the liner of the 114 men and officers on board. After hours of most exhausting efforts and the use of over 40 barrels of lard oil from the Vulcan to still the waters, every man was taken off the Merritt and the Vulcan's tow line ordered cut, at 5 o'clock in the afternoon. The Vulcan and Leonidas remained in the vicinity over twelve hours in order to keep in sight of the Maria Teresa, but she was not again seen till reported to be high up on the coral shoals of Cat Island, 35 miles to the westward of where she was abandoned.

THE FUEL VALUE OF CEREALS. BY PROF. THOMAS GRAY, Rose Polytechnic Institute, Terre Haute, Ind.

The following table gives the names of the substances experimented on and the average result obtained.

Substance.	Heating value in calories.	Percentage of water.
Corn stalk	4030	10.8
Yellow corn	4099	12.1
Yellow corn cob	4015	10.1
White corn	3850	13.0
White corn cob	4065	...
Corn husk	3939	...
Mixed oats	4203	11.0
Wheat	4096	12.8
Rye	3852	12.5
Barley	3807	11.7
Millet	4137	10.5
Rice	3755	13.2
Navy beans	3860	13.8
Wheat straw	4043	...
Timothy hay	4137	...
Cotton seed	5152	7.7
Cotton	4157	...
Sunflower seed	4932	8.8
Castor bean	5405	6.2

THE FRACTURE OF SPHERES. BY PROF. W. T. MAGRUDER, Ohio State University, Columbus, Ohio.

The paper described experiments made on steel balls from 1/8 inch to 1 inch in diameter, broken by compression between parallel steel plates. The different kinds of fractures were described and the broken specimens

and photographs were exhibited. Particular attention was called to the spheres in which two cones were produced at the places where the compression was applied.

A NEW EXACT GRAPHICAL METHOD FOR DESIGNING CONE PULLEYS. BY PROF. W. K. PALMER, University of Kansas, Lawrence, Kansas.

SOME EXPERIMENTS ON COMBUSTION IN LOCOMOTIVE BOILERS. BY PROF. J. W. SHEPHERD, Rose Polytechnic Institute, Terre Haute, Ind.

It was recently desired in connection with thesis work at the Rose Polytechnic Institute to make certain tests upon a locomotive boiler. In order to have the operation of the boiler under perfect control a shop test was decided upon. No suitable means for mounting a locomotive so that the engine could be run were available. Neither was there an absorbing dynamometer of sufficient capacity at hand.

It was not essential that the engines be operated, as the investigation was directed toward the operation of the boiler more essentially. It was desirable, however, that the boiler be operated so nearly as possible under ordinary working conditions, especially as regards the effect of the exhaust strain in producing draft.

The steam valve on one side was disconnected and blocked in middle position so that no steam could enter the cylinder on that side. The locomotive was moved on the track until the piston on the other side was approximately in the middle of its stroke and then the main drivers were securely blocked. At the same time the crosshead was also blocked.

The back-up eccentric rod was then disconnected and the extension piece was bolted to the link for the purpose of connecting with an external source of power for driving the link motion and through it the valve. The power in this case was furnished by an electric railway motor of 15 horse-power. On the end of the electric motor shaft was keyed a forged crank with a one-inch steel crank pin. The pin worked in a link block which in turn slippled in a suitable slot cut in the extension piece to the main link.

By this arrangement the forward eccentric rod pin served as a fulcrum for the link, and a reciprocating motion was imparted to the link by the eccentric motor through the mediums of the crank and slot. The link block of the locomotive mechanism was left in place and imparted motion to the valve in the usual way.

The movement of the valve could be regulated slightly by raising or lowering the link. Additional regulation of the amount of steam discharged was obtained by opening or closing the throttle.

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By these means all the steam from the boiler was discharged into the smoke stack through the blast nozzle in regular puffs or pulsations, and the effect upon the draft was the same as if the engine were in full operation. Since only one steam cylinder and valve were used, the number of times steam was discharged into the stack was only half as great as when the locomotive was in actual use with the same number of valve strokes per minute. However, by increasing the speed of the valve the action of the blast nozzle under running conditions could be very exactly imitated.

The electric motor used was placed directly underneath the locomotive between the tracks in such a position that a crank on the end of the armature shaft was in the proper position to work with the slotted extension to the link. The motor rested on two 6 × 6-inch timbers which were securely bolted to the track. Two other 6 × 6-inch timbers were placed back of the adjacent wheels of the locomotive on either side of the motor and tie rods were run to them from each of the hinge bearings and from the nose of the electric motor. The motor was thus held firmly to resist all tendency to lateral movement or to jumping. The 6 × 6-inch timbers under the locomotive wheels constituted part of the blocking and the use of tie rods as described prevented any loosening of the blocking while a test was being made. While there was no actual movement of the steam piston, yet the admission of the steam cylinder produced a rocking motion quite similar to that of an engine upon the road.

It was found that the sliding block which transmitted the power from the crank pin attached to the motor shaft to the link heated considerable, especially at high speeds and with high steam pressures on the back of the valve. To overcome this difficulty, a bath of oil and water was used to lubricate the crank pin and the sliding block.

The electric motor was supplied with a current at 500 volts and the speed was regulated by the insertion of a salt-water bath as a rheostat in series with the motor.

During some of the tests a steam indicator was connected to the valve chest to determine the pressure upon the back of the valve. The motion of the indicator drum was obtained directly from the valve stem, so that the indicator card gave the pressure on the back of the valve at each point in its movement. The electrical power delivered to the motor was measured by an ammeter and a voltmeter. Separate tests were made to determine the power required to drive the link motion with valve disconnected, also to drive the valve with no steam in the engine chest. The efficiency of the electric motor under various conditions was known from previous tests. Consequently all the data were at hand for determining the coefficient of friction of the slide valve upon its seat.

In addition to the ordinary data taken during boiler trials, measurements were made at regular intervals of the temperature in the fire-box just in front of the flue sheet; also of the draft both above and below the diaphragm plate, in the fire-box and in the ash-pit. Flue gas analyses were

also made, each sample being collected from five different points in front of the front tube sheet.

The pyrometer used to measure the temperature in the fire-box was a modified form of the Siemens Electrical Resistance Pyrometer.

It is not the purpose of this paper to give the results obtained, but simply to describe some novel features of the method used.

SOME THERMAL DETERMINATIONS IN HEATING AND VENTILATING BUILDINGS. BY PROF. C. B. MORRISON, Manual Training High School, Kansas City, Mo.

The heating and the ventilating of buildings are necessarily correlative operations, and must, therefore, be considered together.

The diversity of practice revealed by the study of existing buildings, as well as the architecture of both past and present times plainly reveals that there is no consensus of competent opinion concerning the proper method of applying heat to buildings so as to provide at once a sufficient warmth and a constant change of air for the occupants, and this in accordance with the best economy.

The diverse conditions existing in different buildings in different localities and under varying temperatures as well as the long time necessary to collect reliable data makes it extremely difficult to obtain from these sources anything like accurate comparisons, and this difficulty is augmented by the impossibility of getting reports wholly unbiased and from disinterested parties.

In the experiments here described an attempt is made to bring through carefully prepared apparatus some of the current methods of heating and ventilating into comparison. The apparatus consisted of a model house of one room, Fig. 1, and heated by electricity. *d, d* are fresh air shafts ; *o, o*, openings from fresh air shafts into the indirect radiator box under the floor ; *c*, aspirating flue heated by a tin tube extended from a large lamp chimney, *l* ; *f*, a perforated floor ; *t, t*, Farenheit thermometers extending through corks to top and bottom of the room respectively ; *p*, a strip of white cloth saturated with a slightly alkaline solution of phenolphthalein ; R, drawer containing coils of iron wire heated by the electric current ; *c'*, copper coil for heating air ; B, bellows for forcing air through hot coil.

The method of application and comparison of the heat, of supplying, measuring, and testing the air is shown in Fig. 2, a diagram of the apparatus in place for one of the tests. The quantity of current needed for heating the coils is regulated by the rheostat, R', and measured by the ammeter, A. The room was thus raised to any desired temperature and the quantity of current employed noted. According to Joule's law of equivalents, the amount of heat developed in the circuit is proportional to the square of the current. This brings to hand an accurate means of

thermal comparison.   By considering the factor of resistance, the equivalents in heat units might be definitely ascertained, but as the object of

FIG. 1.—EXPERIMENTAL HOUSE MODEL—FOR HEATING AND VENTILATING.

these tests is to compare the heat used in various methods of heating, it is necessary only to obtain comparative results.   It will be understood, therefore, that the heat units referred to in Fig. 4, are comparative and not absolute.   In the application of the principles here considered, it is to be understood that resistance wires in the model take the place of steam pipes in large buildings, it being alike possible with each to distribute the heat by placing wires or piping just where the heat is wanted.

Ten of the experiments made with this apparatus are tabulated for convenient reference in Fig. 3, and will now be described in detail :

Experiment 1.—Represents direct radiation of stoves or steam pipes placed in the room.   The radiation was furnished by a coil of circuit wire placed in the middle of the room just above the floor.   The holes in the floor were closed by covering it with a close fitting sheet of cardboard. There were no openings except the crack around the door.   In this we

have a case of no ventilation whaetver, a condition often found in school

FIG. 2.—EXPERIMENTAL APPARATUS—EXPERIMENT NO. 9.

and other public buildings—a condition ameliorated only by the mistakes of carpentry in the ill-fitting of doors and windows. The temperature of the room in which the tests were made was 70°; this temperature is the "outside" referred to in the table, and it may here be noted that in all the tests the heat was regulated by means of the rheostat so as to maintain a temperature at the position of the lower thermometer just above the floor of 110°, 40° above that of the outside air. This difference was selected because it represents the average number of degrees the air has to be raised in the northern states during the fire months as given by the Weather Bureau, U. S. A., and Blodgett's "Climatology of the United States." The high temperature, 150°, at the top of the room caused by the hot air rising and being trapped there is a source of waste, as there is usually a rapid escape—due to the differences between interior and exterior pressure—of this hot air through the upper part of loosely fitting windows; it is also a source of headache, as a temperature sufficiently high for the feet will necessitate a temperature too high at the position of the head.

To ascertain the position and relative quantity of carbon dioxide in the

room, a strip of white muslin saturated with a slightly alkaline solution of phenolphthalein was suspended from the ceiling, extending to the floor. The bright red color of this solution when alkaline instantly disappears when neutralized by an acid. The effect of the carbon dioxide in its bleaching effect on the strip watched through the glass door of the room furnishes a good idea not only of the position but of the quantity of carbon dioxide in the room. The carbon dioxide was furnished by burning candles placed on the floor of the room. In five seconds after the strip was put in place, bleaching was perceptible at the top ; it proceeded downward till in about one minute it became completely white at the bottom. The relative amount of carbon dioxide at the top and bottom of

No	Kind of Radiation	Method of Heating	Temperature Fah. Outside 70°		CO2		Outlet	Method of Exhaust	Bleaching Begun		Ended		Air Movement	Current in Amperes
			Above	Below	Above	Below			Min	Sec	Min	Sec		
1	Direct	Coils	150	110			None	None	−	5	1	−	Diffusion	2
2	Direct	Coils	160	110			Below	Window	−	10	1	50	Gravital	3
3	Direct	Coils	115	110			Above	Window	−	20	2	−	"	7
4	Indirect	Coils	215	110			Below	Hot chimney	−	2	1	55	"	6
5	"	Coils	260	110			Above	"	3	−	15	−	"	10
6	"	Hot Air	200	110			Below	Cold / Hot	− / −	5 / 10	1 / 2	10 / −	"	−
7	"	Hot Air	210	110			Above	Hot chimney	4	−	10	−	"	−
8	"	Hot Air	115	110			Below	"	6	−	15	−	Plenum	−
9	"	Coils	105	110			Above	Top outlet	4	−	None	Vacuum	3
10	"	Coils	106	110			Above & Below	Hot chimney	5	−	"	"	Gravital	3

FIG. 3.—TABULATED EXPERIMENTAL DATA.

the room is shown graphically in the table by the relative width of the black area representing from left to right in the table the condition from top to bottom in the room. The difference in temperature of 40° between inside and outside air was maintained by a current of two amperes. Here economy of fuel is obtained at the expense of sanitation.

Experiment 2.—The radiation and method of heating was the same as in Experiment 1. The lower register was opened into the aspirating flue without heat. In maintaining the constant temperature at the bottom of the room, three amperes of current were required, the temperature at the top of the room rising to 160°. Bleaching began in ten seconds at the top, and ended in one minute and fifty seconds at the bottom, showing that the carbon dioxide at the top was greatly in excess of that at the bottom. The only ventilation was that resulting from the gravital ten-

dency of the air at the floor at a temperature of 110° to pass through the register to the outside air at 70°. This represents the condition commonly seen in buildings where a cold flue with an opening at the floor is the only attempt at ventilation. The direction which the air will take in this case will depend on the outside temperature; if it is higher than that on the inside as in summer, the current will be reversed. The increase in the heat required is due to the small outward movement of the air allowed by the opening.

The position of the carbon dioxide shows the common belief, that it is at the bottom, to be erroneous. While heavier than air at the same temperature, at the temperature of combustion carbon dioxide is much lighter than air and rises to the ceiling before diffusion takes place. I have found in other tests made in school buildings and tabulated in "Warming and Ventilation of School Buildings," page 37, that the same is true with carbon dioxide at the temperature of respiration. It will readily be seen that this carbon dioxide when the ventilation is at the floor must pass downward past the breathing line before it can pass out. The ventilation by this means is about the same as that from a small window-opening in calm weather.

Experiment 3.—In this test, the opening into the aspirating chimney was made at the top, all other conditions remaining the same. The rapid escape of the air in its tendency to rise when heated required a current of seven amperes to maintain the temperature of 110° below, but the temperature at the top fell to 115°. The outflow of the air was somewhat checked by there being no inlet—producing cross currents in the flue. The bleaching not being perceptible for twenty seconds shows that the ventilation is here much improved. The bleaching proceeded evenly and required two minutes to complete it. The top ventilation prevented the accumulation of carbon dioxide at this point. The large current required to maintain the standard temperature raised the coil to redness, and represented a wasteful expenditure of fuel. This shows why, in customary methods of heating with the heat undisturbed and coming from a single source, it is necessary to ventilate at the bottom. The heated air rising from a single source must be trapped at the top that it may not be allowed to pass out before it has been used. The error of non-distribution necessitates another error—that of bottom ventilation.

Experiment 4.—In this experiment the heat is furnished by the coils in the rheostat drawer beneath the floor. All the floor openings are closed except those of a single row on the side opposite the aspirating chimney which is opened at the bottom register just above the floor. The ratio of the area of the floor openings to the cubical capacity of the room is about the same as that of current practice. The aspirating chimney is heated by a tin tube extending through it from a large oil lamp. We have here the case of a house heated by the indirect radiation of steam pipes, the air admitted through the floor and limited to one side only and drawn out from the bottom through a heated chimney. By reference to the table, it may be observed that six amperes were required and that the

temperature at the top rose to 215°. The bleaching shows the carbon dioxide to be present in largest quantities at the top, and the time required indicates an imperfect ventilation. The high temperature at the top of the room caused by the want of ventilation at that point and the expenditure of heat shown by six amperes—the reading on the ammeter—shows this to be an expensive and ineffectual method.

Experiment 5.—The only change in the apparatus in this experiment was the closing of the lower foul air register and the opening of the upper one. The position of the carbon dioxide was reversed, being at the bottom instead of at the top of the room, the slow bleaching showing that the gas was passed out freely with the air at the top. But the expense of this method was shown by the ten amperes of current necessary to maintain the normal difference of 40° between lower inside and outside. The temperature at the top was high, 260°, the ventilation good, the flow of the air through the room, rapid, and the expenditure of heat enormous. This again goes to show the impracticability of ventilating at the top when the air is not properly distributed as it enters the room.

Experiment 6.—Hot air was now substituted for the coils and admitted as is customary in hot-air systems at the side of the room about two-thirds of the distance from the floor to the ceiling. The air was heated by a sheet-iron drum surrounded by a zinc jacket terminating in the tube leading to the room. The heat was furnished by a small oil-stove and regulated by turning the flame up or down, till the 110° temperature at the bottom of the room became constant. It may here be noted that in all these experiments the test strip for carbon dioxide was not put in place till this constant temperature had been secured.

The opening into the aspirating chimney was below as is the common practice in hot-air systems. The experiment was made in two ways : 1. With the aspirating chimney cold ; and 2. With the chimney heated. When cold, the bleaching began in five seconds and in ten seconds when heated, ending in one minute and ten seconds in the first case and in two minutes in the second case. This shows the merit of heating the aspirating chimney. In both cases, the carbon dioxide was seen to be present in largest quantity about half way between the floor and ceiling. The explanation of this appears to be that the very hot air delivered at the top was specifically lighter than the carbon dioxide. It was also probably due to the transverse current of hot air across the upper part of the room as it passed across on its way to the opposite side before starting downward toward the outlet. This position of the carbon dioxide at or a little above the breathing line of the occupants of a room in hot air systems agrees with my school-room tests made in 1886. The method of supplying the heat in this experiment forbids any accurate comparison of its amount with those of the preceding ones, but it is certain that owing to the high temperature at the top of the room and the high heat to which it was necessary to raise the drum in order to maintain the standard difference in temperature between the outside and lower inside, it must be great and the expenditure of fuel excessive.

Experiment 7.—Here the upper foul air register was opened and the lower one closed. The temperature at 110° at the lower part of the room was maintained with much difficulty, the air at 210° at the top passing out before mixing with that below. The bleaching requiring ten minutes showed that the carbon dioxide was rapidly carried away. This again shows the impracticability of ventilating at the top when the hot air is not distributed before its admission to the room.

Experiment 8.—In this experiment a coil of copper tubing was substituted for the drum and heated over the flame of a large gas burner. Air was forced through this tube by means of a hand bellows, B, as shown in Fig. 2. With ventilation below and heat in the aspirating chimney, the lower temperature could easily be maintained with air entering near the top at 115°. Bleaching was uniform from top to bottom, beginning in six minutes and ending in eleven minutes. Here is seen the advantage of the plenum over the gravital movement of the air. The air carried rapidly over the heated surfaces does not become overheated, and it warms the room more evenly and with frequent changes. This plenum method of heating and ventilating in current practice in many of our large cities is operated by forcing air over steam coils by means of engine-driven fans, and represents an important step in advance of gravital systems as usually installed.

Experiment 9.—The drawer rheostat was put in place under the floor and all the floor openings opened. The outlet registers were both closed, and the air taken out through a tube from the top of the room, and the time of a complete change of air ascertained by drawing it through a Bunsen filter-pump rated so as to effect a complete change of air in one minute. This rate was secured by first drawing the air from a glass jar of known capacity inverted over water allowing it to fill by displacement and noting the time. The temperature at the top of the room was only 108° while steadily maintained at 110° at the bottom. The bleaching was slightly noticeable at the top but took place so slowly as to indicate practically perfect ventilation. The normal temperature was maintained with a constant current of three amperes. The efficiency and economy of this arrangement results chiefly from the perfect distribution of the heat beneath the floor and the numerous small registers allowing the air to rise bodily and slowly to the top of the room where it, having been utilized, is ready to be let out. The floor is warmed and itself becomes a radiator. The highest temperature is at the position of the feet of the occupants, and the coolest, the position of the head—a condition in conformity with hygienic requirements. The carbon dioxide is carried off as fast as formed and not drawn down past the breathing line as in cases in which it is trapped at the top of the room and subsequently drawn out at the bottom. The distribution of steam pipes beneath the floor is simply an extension of the plenum or hot air chamber and involves no mechanical difficulties. An approach toward this complete distribution of the heat is made in the Kansas City Manual Training High School in which the plenum room is extended beneath the floors of the corridors obviating

the use of distributing pipes in the basement. A large plenum fan forces the air from this extended chamber to the rooms and is supplemented by an exhaust fan in communication with the separate rooms.

Experiment 10.—After making a great number of combinations and tests to ascertain whether the perfect results shown by Experiment 9 could be secured without the aid of mechanical means, the conditions tabulated in Experiment 10 were found. Here the coils were left in place beneath the floor with floor openings all open. The ventilation was made by opening the lower exit register and by partially opening the upper one into a heated aspirating chimney.

The standard temperature was thus maintained with three amperes the same as before, and the air as indicated by the test strip was kept equally pure. The temperature at the top of the room was only 106°. While the outflow could not in this case be measured, it may be inferred from the ammeter reading and the test strip that it was about the same ; *i. e.*, a complete change of air was effected once every minute. This rate of change is beyond the requirements for actual practice, once every six being considered sufficient.

The ten experiments here recorded are only a few of those which have been made during the past five years. But these have been selected as fairly representing the methods of current practice. While making these tests, it was discovered that the relation between the ammeter readings, the temperature the air is raised, and the number of resulting heat units may be shown in the plotted diagram, Fig. 4. As a single illustration of its use, I have selected three experiments which show the enormous waste of fuel in some of our systems of heating. The figures at the left show the difference between inside and outside temperatures ; those at the top, amperes of electric current used in heating iron coils as the source of heat; those at the bottom, relative heat units. It will be noted that these are the squares of the amperes above and thus show the thermal relation—before mentioned—between the current and its thermal equivalent, it being remembered that these numbers are not thermal units, but serve to show the relative amount of heat at different readings of the ammeter. The line AO shows the results when the air was distributed under the floor with ventilation above ; BO, when the air was delivered at the side with ventilation below ; CO, when the air was delivered near the top and let out at the top. Take an example : Suppose the temperature above that outside of the room to be 50 ; this temperature line crosses the resultant line at X, showing that it requires two and one-half amperes of current to maintain this temperature when heat is applied below. With the same temperature when the heat is applied at the side, the line crosses at B, showing ten amperes, whence it is plain that the relative heat required in two cases is shown by the ratio of six and one-fourth to one hundred. In plain words, it would require only six and one-fourth per cent. of the cost by present methods to heat a building if the air were properly distributed, delivered through the floors, and let out at the top. I am aware that the inertia of architectural convention and the persistence

of current custom will restrain architects from a serious consideration of this method of heating for some time to come. It will be useful, therefore, to consider the best that can soon reasonably be hoped for without the distribution of a perfect system, and to consider mechanical power as a means of heating and ventilating. The necessity of this means in very large build-
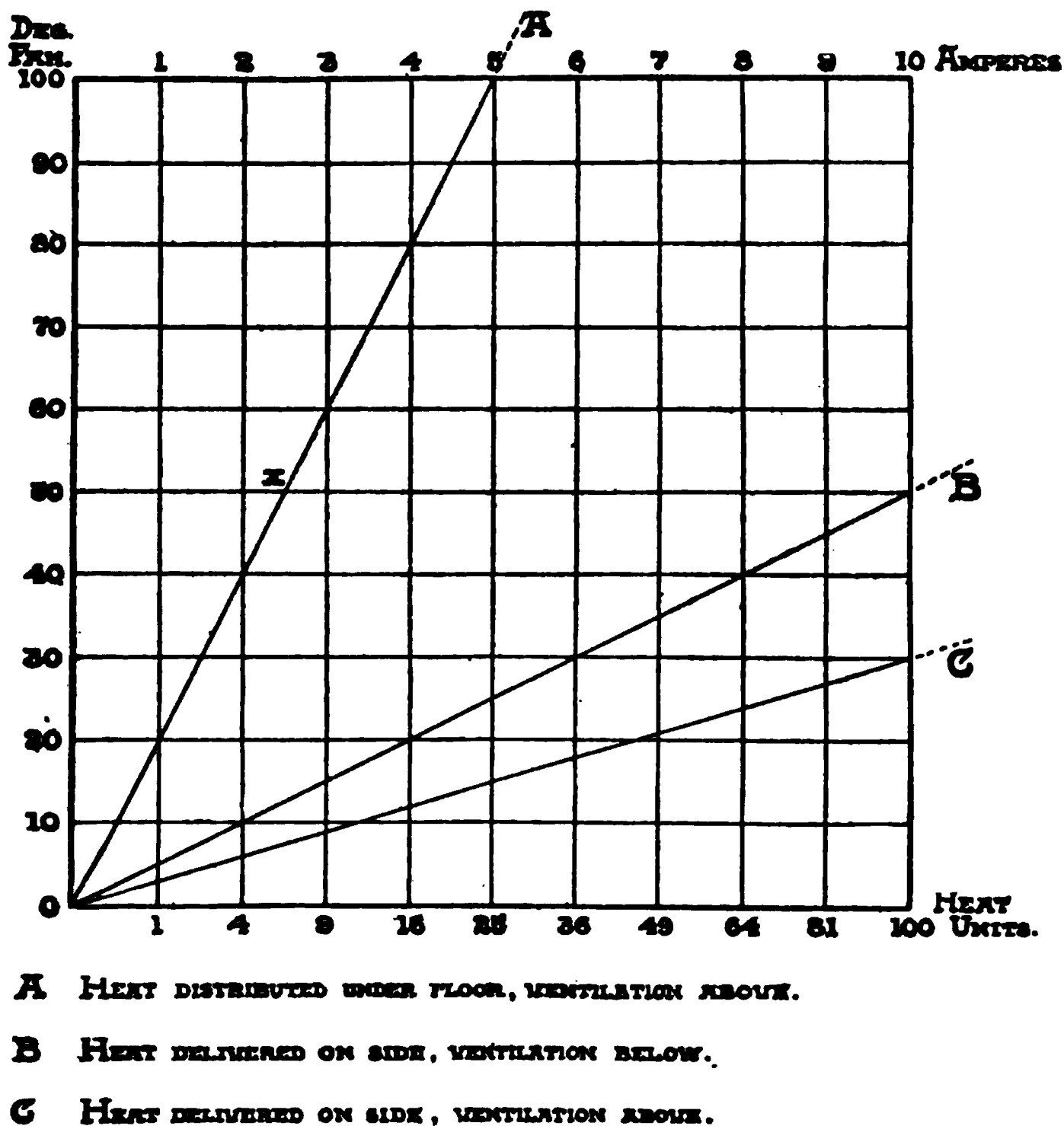
FIG. 4.—PLOT OF COMPARATIVE NUMBER OF HEAT UNITS.

ings is no longer a subject of debate and is in use in all first-class schools and other public buildings in the large cities; but it is generally supposed that to buy an engine and fans for ventilating an ordinary eight-room school building would be an expensive luxury. This is not only an error, but it may be safely said that the converse is true—that it is expensive to do without engine and fans. For illustration, I will submit a few statements which have recently been verified in observations made in the heat-

ing plant in the new Manual Training High School, Kansas City, Mo.:

It are now generally accepted that 2000 cubic feet of air at normal pressure are needed for each occupant per hour if the requirements of perfect ventilation are met; but the mistake is commonly made that this amount is ever realized in systems of gravity ventilation where the air is moved by heating aspirating chimneys. It is not denied that this quantity of air per person can be moved by the gravity method; only that it is not done in practice.

The most careful estimates place the amount of fuel necessary for this purpose as about one-sixth in excess of that required to supply the heating. So that to ventilate a building properly by the gravity method, more than doubles the cost of heating without ventilation. It is plain that the burning of such large quantities of coal in chimneys for the purpose of ventilation is expensive and—in view of a better way—wasteful.

Without burdening you with deduction formulas, it may be reliably asserted that each pupil in a school may be supplied for a whole school year with 2000 cubic feet of air per hour at a cost less than one cent. As this statement will be reluctantly accepted by many who are unfamiliar with such matters, a few words of explanation will not be out of place.

It should be remembered that in securing this result the exhaust steam is not wasted but is admitted directly into the radiators and utilized for heating the building. The engine simply converts enough of the steam as it passes through into mechanical power to run the fans. The drop in the temperature of the steam which this change causes is very small, so small indeed that it might almost be neglected, and it is this drop which supplies the entire expenditure for ventilation.

In the complete combustion of a single pound of average bituminous coal there is liberated 13,000 heat units; multiplying this by the mechanical equivalent, 772, we get 10,036,000—the number of foot-pounds of actual work of which one pound of coal is capable when the transformation takes place without loss; and this is precisely the case when a fan is run by an engine and the exhaust steam used for heating the building.

It will be interesting to note that this work, 10,036,000 foot pounds, when divided by 33,000, the horse power per minute, gives 304 plus, as the number of minutes one pound of coal will supply a horse power of work. One horse power is the work necessary to ventilate an average classroom. We see then that one averaged sized schoolroom can by this means be amply ventilated for five hours with only one pound of coal. At $4 per ton, this would cost one-fifth of a cent!

To move air at the same rate by burning coal in a ventilating chimney, it would require for the same time an average of 100 pounds of coal; thus the cost of mechanical ventilation is only 1 per cent. of that equally well done by gravity. To ventilate an eight-room building by mechanical means, would require an eight horse-power engine and two three-foot fans. The cost of an instalment would not exceed $350.

Twenty-one pounds per hour is the quantity of coal which careful estimates place as necessary to ventilate a schoolroom containing sixty

pupils. Now counting seven the number of fire months, twenty the number of days to the month, eight as the number of hours per day in which fire will be needed, $4 the price of a ton of coal, the cost of ventilating a building of eight rooms would be $\dfrac{7 \times 20 \times 8 \times 8 \times 21 \times 4}{2000} =$ $376.32.

Any less expense would imply that the ventilation is imperfect and short of that which would be supplied by engine-driven fans. Thus, a power plant would pay for itself in one year in the saving of coal alone.

But there are other compensations incident to this system in the installation. It should be remembered that all ducts, both for fresh and for foul air need to be only half the size of those for gravity ventilation ; this is because of a corresponding difference in the velocity of the air in the two systems.

Again, the indirect radiating surface is at least one-third less, due to the higher steam pressure which may be carried to supply the drop in temperature which takes place on radiator surfaces when strong currents are passed over them.

Taking then, the great daily saving in coal consumption, the trifling extra expense of first installation, and the uncertainty of the action and efficiency of the gravical method, what remains to be said? Simply that in buildings of eight rooms and upwards, mechanical ventilation should take the place of gravital. Whether we consider the matter from an hygienic, economic, or mechanical basis, this conclusion seems to be inevitable—a conclusion which has been amply verified by the writer in observations at the Kansas City Manual Training High School Plant during the past two years.

PNEUMATIC SYSTEM OF PREVENTING THE BURSTING OF WATER-PIPES THROUGH FREEZING. BY N. MONROE HOPKINS, Washington, D. C.

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ADDRESS

BY

J. F. WHITEAVES,

VICE-PRESIDENT AND CHAIRMAN OF SECTION E.

THE DEVONIAN SYSTEM IN CANADA.

To the student of the early literature of the Palæozoic rocks, and especially to the palæontologist, the name of William Lonsdale will always be associated with the Devonian System.

Although the term Devonian was first definitely proposed by Sedgwick and Murchison in a paper read April, 1839, and published in the fifth volume of the second series of Transactions of the Geological Society of London, the authors of this paper are careful to state, (first) that " Mr. Lonsdale, after an extensive examination of the fossils of South Devon, had pronounced them, more than a year ago, to form a group intermediate between the Carboniferous and Silurian systems," and, (secondly) that "the previous conclusions of Mr. Lonsdale .... led the way to their proposed classification of the Cornish and Devonian formations."

Lonsdale, himself, in another paper printed in the same volume, distinctly claims that his suggestion, on the evidence of their fossils, that the South Devon limestones are "of an intermediate age between the Carboniferous and Silurian systems, and consequently of the age of the old red sandstone," was first made in December, 1837. S. P. Woodward, in the preface to the first part of his "Manual of the Mollusca," dated

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March, 1856, speaks of Lonsdale as his "friend and master, *the founder of the Devonian system in geology.*"

Yet, so lately as in August, 1897, Mr. Marr is stated to have said[1] that "the Devonian system had been founded on strati-graphical grounds by Murchison and Sedgwick, and on palæon-tological grounds by Lonsdale and Etheridge." Surely it would have been more correct to have said that the existence of the Devonian as a distinct geological system was first indicated by Lonsdale in 1837 on purely palæontological evidence, and subsequently confirmed by Sedgwick and Murchison in 1839 on stratigraphical considerations.

However this may be, rocks of Devonian age have been discovered at various times in almost every province and district of the Dominion, and it is thought that a brief summary of the history of these discoveries and of the present state of our knowledge of the Devonian rocks of Canada, from a palæontologist's point of view, may be of interest on this occasion. In accordance with long usage in Canada, the line of demarcation between the Silurian and Devonian systems, in this address, will be drawn at the base of the Oriskany sandstone. It will also be convenient to consider the information that has so far been gained about the Devonian rocks of Canada in geographical order, from east to west, under the three following heads, *viz.*: (1) The Maritime Provinces and Quebec; (2) Ontario and Keewatin ; and (3) Manitoba and the North-west Territories.

I. THE MARITIME PROVINCES AND QUEBEC.

Nova Scotia.

In a memoir accompanying a geological map of Nova Scotia, by Dr. Abraham Gesner, published in the Proceedings of the Geological Society of London for May 10, 1843,[2] the following paragraph occurs :

"*Old red sandstone or Devonian group.*—Above the Silurian beds there occurs, in several parts of the province, a bright red micaceous sandstone or conglomerate, accompanied by thin beds of red shale and marly clay, and in some places containing seams of fibrous gypsum. Hitherto no organic remains have been found in it. At Advocate Har-

[1] Quarterly Journal of the Geological Society of London, vol. LIII, page 460.
[2] Volume IV, part I, page 187.

bour and on the Moose river this sandstone is seen lying unconformably beneath the coal measures. At the latter locality the sandstone dips W. 21°, and the coal measures dip N. N. E. 60°. It is from a joint consideration of the mineral characters of this formation, and its relative position as compared with the coal measures, that the author has regarded it as the equivalent of the old red sandstone."

This would seem to be the earliest statement in regard to the occurrence of rocks of Devonian age in British North America, but Gesner then included in his old red sandstone group, certain outliers of carboniferous limestone and possibly trias, that are now known to be associated with rocks still held to be Devonian.

Not quite two years later than this, in a paper read before the Geological Society of London on January 22, 1845, Sir William Dawson says that beyond Cape John the newer coal formation "seems to overlie, unconformably, a series of hard grits, slates and limestones, with scales of *Holoptychius*, Encrinites and fragments of bivalve shells, and which are probably of newer Silurian or Devonian age. The last-mentioned rocks, with various kinds of trap, form an elevated ridge belonging to the Cobequid chain of hills."[1]

Influenced, as he elsewhere tells us, by information supplied by Sir Charles Lyell, Gesner's earlier statements as to the Devonian rocks of Nova Scotia were modified in his "Industrial Resources of Nova Scotia," published at Halifax in 1849. In this volume the paragraph about the Devonian rocks is as follows :

"Old Red Sandstone or Devonian Group.—Above the Silurian strata there occur thick beds of conglomerate, bright red, and micaceous sandstones, red shale and marly clay. At Advocate Harbour, Parrsboro', Moose river, Horton, Shubenacadie, and other places, these rocks are seen dipping beneath the coal measures and gypsiferous red sandstones. The scales of fishes, and other organic remains, found in these deposits, are too scanty and imperfect to afford conclusive evidence of their relative age ; but from a joint consideration of them, the mineral character of the formation and its position, it may be classed as the equivalent of the old red sandstone of Europe, or a part of the great carboniferous series. The strata contain but few minerals of importance."

The first edition of the " Acadian Geology," by Sir William Dawson, published in 1855, contains a " Tabular View of Rock

[1] Quarterly Journal of the Geological Society of London, vol. 1. page 235.

Formations in Nova Scotia," in which the Devonian is defined
as including the "fossiliferous slates of Bear river, Nictaux,
New Canaan, Pictou, Arisaig, etc., and perhaps also parts of
the metamorphic rocks of the Cobequid and Pictou hills." In
the fourteenth chapter of this volume the fossiliferous slates at
Arisaig and the East river of Pictou are regarded as of Devo-
nian age, on the authority of James Hall, but in a supplement-
ary chapter, dated August, 1860, they are referred to the
Silurian. Nine " fossils from the Devonian and upper Silurian
(?) rocks of Nova Scotia " are figured in this volume, but
none of these are specifically determined and only three are
Devonian. But, in the supplementary chapter, four of the
fossils of the Nictaux and one of the Bear River series are de-
termined specifically. Of the former it is stated that Hall
"compares them with the fauna of the Oriskany sandstone; and
they seem to give indubitable testimony that the Nictaux iron
ore is of Lower Devonian age." A fuller list of fossils from
Bear River and Nictaux, in which sixteen species are described
generically and nine specifically, was published in 1891.[1]

In the second and much enlarged edition of the " Acadian
Geology," published in 1868, Sir William Dawson confirms
and elaborates most of the statements about the Devonian of
Nova Scotia in the first edition and " Supplementáry Chapter,"
and figures a new Devonian Spirifer (*S. Nictavensis*) from Nic-
taux.[2] He notes the occurrence of "obscure remains, evi-
dently of land plants" in more or less altered rocks on the
flanks of the Cobequids, etc., and more particularly the dis-
covery, in 1866, of " stipes of ferns, apparently of two species,
a *Pinnularia*, and branching stems much resembling those of
Psilophyton, a characteristic Devonian genus," in a gray altered
sandstone or quartzite underlying unconformably a Carboni-
ferous conglomerate at Bear Brook (now known as McCulloch
Brook) near the Middle River of Pictou.

Doctor Honeyman, in a paper read before the Nova Scotia
Institute of Natural Science in November, 1870, and since
published in its Transactions, describes, as of Devonian age,
a red band of argillites on McAra's and McAdams' brooks,

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[1] Acadian Geology, Supplementary Note to the Fourth Edition, pp. 20 and 21.
[2] Page 499, figs. 176, *a. b.*

near Arisaig, which he calls the " McAra's Brook strata," but in which he did not succeed in finding any fossils. Later collectors, however, have been more successful, and in 1885, Mr. T. C. Weston, of the Canadian Geological Survey, obtained from these argillites "fragments of plants and fish teeth not certainly determinable, together with certain interesting" imprints "like those of *Protichnites carbonarius.*"[1] From the same rocks, in 1897, Dr. Ami and Mr. Hugh Fletcher, of the same survey, collected fragments of *Pterygotus*, and of Pteraspidian and other fishes. The fish remains obtained in these rocks in 1897 have been examined by Mr. A. Smith Woodward, of the British Museum, who thinks that they are either uppermost Silurian or lowermost Devonian.

From 1872 to the present time Mr. Fletcher has been engaged in a minutely detailed examination of the geological structure of northern and eastern Nova Scotia, for the Geological Survey of Canada, which has published geological maps of the greater portion of this area on a scale of one mile to the inch. In 1887 he referred to the Devonian system the rocks below the Carboniferous conglomerate at Loch Lomond, Richmond County, Cape Breton.[2] From that point he has since traced rocks that he has described as Devonian, on stratigraphical and lithological grounds, westward through the peninsula of Nova Scotia as far as the head of Cobequid Bay and along both sides of Minas Basin, where he has estimated that they attain a thickness of from 10,000 to 15,000 feet.[3] With some Silurian and the associated igneous rocks, he believes them to form the mass of the Cobequids.

Most of these rocks that Mr. Fletcher refers to the Devonian, had, however, previously been referred to other geological horizons. Among the more notable of these are the Horton series in Kings County, and the Riversdale series and Harrington river rocks in Colchester county. On purely palæontological evidence the Horton series had been referred to the Lower Carboniferous, and the Riversdale series to the Millstone Grit, by

1 Geological and Natural History Survey of Canada, Annual Report, new series, vol. II, page 68 P.

2 Geological Survey of Canada, Report of Progress for 1877-78.

3 See the Annual Reports of the same Survey for 1877-78, 1879-80-81, 1886, and 1890-91.

Sir William Dawson, though it is now pretty generally conceded that both are unconformably overlaid by a marine carboniferous limestone.

Owing to circumstances it has unfortunately happened that very little palæontological work has been done in Nova Scotia or on Nova Scotian material since 1873. With the view of stimulating the prosecution of researches in this direction, collections of fossils have been made, during the past four years and chiefly by Dr. H. M. Ami, of the Geological Survey of Canada, from many localities in the province, and some selected sets of these fossils have been forwarded to specialists.

In the Christmas and New Year's week of 1897 and 1898, Mr. David White, of the United States Geological Survey, examined the fossil plants from Nova Scotia and New Brunswick in the Peter Redpath Museum at Montreal, and in the Museum of the Geological Survey at Ottawa. On the evidence of these plant remains Mr. White came to the following conclusions, which are summarized, by permission, from an unpublished report, in the form of a letter addressed to Dr. H. M. Ami, and dated January 12, 1898. (1). That the plant-bearing portion of the Horton series of Nova Scotia, as shown by Sir William Dawson in 1873, is nearly contemporaneous with the Pocono formation of the Eastern United States, which has long been assigned to a basal position in the Carboniferous system. (2). That the Riversdale series of Nova Scotia (which Sir William Dawson referred to the Millstone Grit) is of Carboniferous age and assuredly newer than the Horton series. (3). That the plant-bearing beds near St. John, New Brunswick, are not Middle Devonian, as had previously been supposed, but Carboniferous, and that they are the exact equivalents of the Riversdale series of Nova Scotia.

Early in January last, collections of fossil plants from the Horton and Riversdale series and Harrington river rocks, at several localities in Nova Scotia, were sent to Mr. R. Kidston, of Stirling, Scotland, an experienced palæobotanist, for examination and study. In a manuscript report upon these collections, addressed to the Director of the Canadian Survey, and received May 8, 1899, Mr. Kidston comes to almost exactly the same conclusions as those previously arrived at by Mr.

White, and on perfectly independent grounds. In this report Mr. Kidston expresses the following opinions. 1. Of the Horton series he says: "These rocks appear to be undoubtedly Lower Carboniferous." "There is no evidence at all to support the opinion that they are of Devonian age." "All the evidence derived from a study of their fossils, points very strongly against this view." 2. Of the Riversdale series he says: "The two divisions of this series, the Riversdale Station and Harrington river rocks, may be treated together, as they contain the same fossils and are evidently of the same age." The whole of the fossil plants from the Riversdale series have a most pronounced Upper Carboniferous facies and markedly possess the characteristics of a Coal Measure Flora. "Judged from a European comparison, no other conclusion can be arrived at." 3. Lastly, he says that "the question of the age of the Riversdale series is inseparably connected with the question of the age of the plant beds of St. John, New Brunswick." "The species contained in the Riversdale series are also met with in the St. John plant beds, where, however, a greater number of species has been discovered." "I do not," he adds, "wish to express my views as to the age of the St. John plant beds too strongly, but from what I have been able to learn from a study of the literature of the subject and an examination of specimens from these beds, it appears to me that they possess a flora of a much higher horizon than that assigned to them, and that in reality they are most probably Upper Carboniferous." "It must, however, be remembered that since Sir William Dawson wrote his work on the Pre-Carboniferous Flora, very much has been done in Europe to work out the zones of the Coal Measure Flora, and careful and accurate figures have been published which did not exist at the time he was carrying out his investigations." "A thorough revision of the work, especially in the light of subsequent collections and possible discovery of more perfectly preserved specimens seems most desirable, and also that a better series of figures be published."

As complete a collection as possible of the fish remains of the Horton and Riversdale series of Nova Scotia was sent to Mr. A. Smith Woodward in January, 1899, for examination and

study, but no report upon these specimens has yet been received.

The Devono-Carboniferous problem in Nova Scotia and New Brunswick is far too complicated a question to be discussed at any length in an address of this kind. At present, however, it is obvious that there is some discrepancy between the views of the two geologists on the Canadian Survey staff who have studied the question from a stratigraphical and lithological point of view, and those of the palæontologists whose names have been cited in this connection, as to the age of the Horton and Riversdale series of Nova Scotia, and of the plant-bearing beds near St. John, New Brunswick.

New Brunswick.

It would appear that Devonian rocks, or at any rate, rocks that have for many years been regarded as of Devonian age, were not recognized in New Brunswick until 1861. For, although Dr. Gesner made extensive geological explorations in the province last named, from 1838 to 1843, the strata that he refers to the old red sandstone, in his first report on a Geological Survey thereof, published in 1839, and in a short paragraph in chapter eleven of his volume on New Brunswick, published in 1847, are now regarded as of Carboniferous age.

The occurrence of fossil plants in rocks near St. John, was noticed by Dr. Gesner as early as in his second report on the Geology of New Brunswick, published in 1840, and Sir William Dawson states that a well-characterized specimen from these rocks, which he subsequently identified with the *Calamites transitionis* of Goeppert, was shown to him by the late Professor Robb in 1857.[1]

In 1860 a small collection of fossil plants from the shales at the foot of the city of St. John, near the barracks, recently made by Dr. G. F. Matthew, was submitted to Sir William Dawson for examination. On the evidence of their fossil plants these rocks at St. John were referred to the Devonian system by Sir William, in a paper "on the Pre-Carboniferous flora of New Brunswick, Maine, and Eastern Canada," published in the "Canadian Naturalist and Geologist" for June, 1861. Seven species are recognized in this collection, six of

[1] Acadian Geology, Second Edition, page 502.

which are described as new. Professor L. W. Bailey in his
Report on the Geology of Southern New Brunswick, says that
"the same author, in June, 1861, after an examination of cer-
tain fossils in eastern Maine, asserted the Devonian age of the
rocks containing them, and also of the sandstones constituting
the peninsula of St. Andrews, which they closely resemble."

Immediately after this, rocks containing similar fossils and
presumably therefore of Devonian age, were recognized at
other localities in the neighborhood of St. John, or in St. John
county, as at the Little and Mispec rivers, and more particu-
larly at the Fern Ledges, in Lancaster parish. From the lat-
ter locality extensive collections of fossils were made by Dr.
Matthew, Professor Hartt and other local collectors in 1861,
1862 and 1863, and more recently by Mr. W. J. Wilson and
Dr. Matthew. The luxuriant and singularly varied fossil flora
of the Fern Ledges has been described by Sir William Dawson
in 1862,[1] by Professor Hartt in 1865,[2] by Sir William Dawson
and Professor Hartt in 1868,[3] and by Sir William Dawson in
1871[4] and 1882.[5] The "revised list of the Pre-Carboniferous
plants of N. E. America" in the first part of Sir William's
memoir on "the fossil plants of the Devonian and Upper Silu-
rian formations of Canada," published by the Dominion Sur-
vey in 1871, contains the names of seventy species of fossil
plants from the Devonian of New Brunswick, nearly all of
which are from the Fern Ledges. In the second part of the
same memoir, published in 1882, two additional species were
described.

The remarkable assemblage of air-breathing articulata and
mollusca associated with these plant remains has been described
by Salter in 1863,[6] by Scudder in 1868,[7] by Sir William Daw-
son in 1880,[8] and by Dr. Matthew in 1888[9] and 1894.[10] In the

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[1] Quarterly Journal of the Geological Society of London, vol. XVIII, pp. 296-330.

[2] In an Appendix to Prof. Bailey's Report on the Geology of Southern New
Brunswick.

[3] Acadian Geology, Second Edition, pp. 534-556.

[4] Geological Survey of Canada. Fossil Plants of the Devonian and Upper Silu-
rian formations of Canada.

[5] Ibid., part 2.

[6] Quarterly Journal of the Geological Society of London, vol. XIX, pp. 75-80.

[7] Acadian Geology, Second Edition, pp. 523-526.

[8] American Journal of Science, vol. XX, p. 413.

[9] Transactions of the Royal Society of Canada, vol. VI, sect. 4, pp. 57-62.

[10] Ibid., vol. XII, sect. 4, pp. 95-100.

latter of these two papers Dr. Matthew states that the "air-breathing articulates of the plant-bearing bed of St. John so far recognized, consist of :

"Insects, nine species of eight genera..................... 9
Myriapods, six species of several genera............ 6
Arachnid similar to Anthracomartus.................... 1
Probable pedipalp. (Eurypterella)..................... 1
Probable Arachnid or Isopod (Amphipeltis)............ 1
Scorpion (Palæophonus arctus)........................ 1

"Two species of land snails have also been found, raising the number of air-breathing animals found in the plant-beds at St. John to twenty-one kinds."

Elsewhere in this paper Dr. Matthew says that "later discoveries lead the author to think that *Eurypterus pulicaris*, Salter, should be referred to the myriapods or to the insects," and in the foregoing list it is evidently included with the insects. To this list, also, should be added a trilobite and an annelid (*Spirorbis Erianus*, Dawson), which indicate marine or at least brackish water conditions, while from the description and figures it is difficult to see in what respects the very imperfect specimen described as a land shell under the name *Strophites* (since changed to *Strophella*) *grandæva*, differs from the presumably marine genus *Macrocheilus*.

Detailed descriptions of the stratigraphical relations of the presumed Devonian rocks near St. John, by Dr. Matthew, were published in 1863[1] and 1865,[2] and many additional facts in relation thereto are contained in Professor Bailey's report on the Geology of Southern New Brunswick published in 1865. In 1863 Dr. Matthew gave the local and provisional names of the Mispec, Little River and Bloomsbury groups to the subdivisions of the supposed Devonian system in St. John county, the Little River group including both the Cordaites shales at the Fern Ledges, with their numerous fossil plants, insects, etc., and the Dadoxylon sandstone. The Little River group was at first supposed to be of Upper Devonian age, but in consequence of the investigations of Professor Bailey and Dr. Matthew in 1870, Sir William Dawson, in 1871, expressed the opinion that the Mispec group represents the Upper Devonian, the Little

1 Canadian Naturalist and Geologist, vol. VIII, pp. 241-259.
2 Quarterly Journal of the Geological Society of London, vol. XXI, pp. 429-30.

River group the Middle Devonian, and the Lower Conglome-
rates (presumably the Bloomsbury group) the Lower Devonian.
Matthew, in 1888, after stating that there is one unconformity
between the Perry sandstone and the Mispec beds, and another
between the Mispec beds and the Cordaite shales, thus re-
divides the Devonian rocks of St. John county, the unconform-
ities being marked by a dividing line.

> " Perry Sandstones, with Upper Devonian flora according
>   to Sir J. W. Dawson, but lithologically resembling the
>   Lower Carboniferous sandstone.
>
> ----
>
> " Mispec Conglomerate and slate.
>
> ----
>
> " Cordaite shales and flags, Middle Devonian flora. *Insect
>   remains* (in oldest beds of the Cordaite shales).
> " Dadoxylon sandstone (with an older Devonian flora,
>   G. F. M.).
>   Bloomsbury Conglomerate, etc."[1]

On behalf of the Canadian Survey, in 1870, Professor Bailey
and Dr. Matthew traced beds corresponding to the plant-bear-
ing beds near St. John as far to the westward as Lepreau Har-
bour in Charlotte county, where many fossil plants like those
at the Fern Ledges were collected. Ten years later the distri-
bution of the Devonian rocks in the southern part of the pro-
vince, as far as then known, was thus summarized by Messrs.
Bailey, Matthew and Ells.

" The areas of Devonian occurring in southern New Brunswick may be
stated as follows :

" 1. A large basin, or double synclinal, east of St. John harbour, occu-
pying the valley of the Mispec, with a southern area extending north-
easterly across the Black River, near the forks of the East Branch.

" 2. Isolated outcrops on Coal Creek and on Canaan River and North
Fork, presumably of this age, but lacking evidence of fossils.

" 3. Small areas about St. John and Carlton, with possibly Partridge
Island.

" 4. A small area about the eastern extremity of Spruce Lake, on the
St. Andrews railroad.

" 5. A belt stretching west from Musquash Harbour to Lepreau Har-
bour, in which is contained the so-called anthracite mine of Belas Basin,
with a smaller detached area along the shore from By Chance Harbour to
Dipper Harbour.

[1] Transactions of the Royal Society of Canada, vol. VI, sect. 4, p. 61.

"6. A large area in the northern part of Charlotte county, embracing the former pale argillite series and extending into Queens county."[1]

Prior to 1894 the Devonian age of these rocks had never been called in question. But, in a foot-note to page 79 of Sir William Dawson's "Synopsis of the Air-breathing Animals of the Palæozoic Period in Canada up to 1894," published in the Transactions of the Royal Society of Canada for that year, Dr. Matthew says of the Little River group (which includes the plant-bearing beds near St. John) that he has "recently found some reason to suspect that these beds are as old as Silurian." And, as already stated in connection with this phase of the Devono-Carboniferous problem in Nova Scotia, both Mr. White and Mr. Kidston, on the evidence of their plant remains, have independently and quite recently expressed the opinion that the plant-bearing beds near St. John are the exact equivalents of the Riversdale series of the Nova Scotia Carboniferous.

In northern New Brunswick an area of gray shale (with *Psilophyton*) and conglomerates, which are regarded as of Devonian age, on the east side of the St. John River, near the mouth of the Beccaguimic, is indicated in a map accompanying Dr. Ells' "Report on the Iron Ore Deposits of Carleton County," in the "Report of Progress of the Geological Survey of Canada for 1874-75." Dr. Ells, also, in the "Report of Progress" of the same survey for 1879-80, says that areas of Devonian rocks are "seen at intervals along the lower Restigouche River," and that they "form a synclinal basin extending from near the town of Dalhousie westward to a point about two miles above Campbellton and terminating on the south side of the river at Old Mission Point." This report is descriptive of explorations made in 1879, and in it the Devonian age of the rocks at Campbellton is assumed exclusively on the evidence of a few fossil plants (*i. e.* two species of *Psilophyton*, one of *Lycopodites* and one of *Cordaites*) that had been identified or described by Sir William Dawson. The remarkable fish-fauna at Campbellton was not discovered until June 27, 1881, but it will be more convenient to consider it later on, in connection with the equally notable fish-fauna discovered in 1879, on the opposite

[1] Geological Survey of Canada, Report of Progress for 1878-79, page 11 D.

side of the lower Restigouche River at Scaumenac Bay in the Province of Quebec, as the two localities are only about sixteen miles apart. Another area of Devonian rocks in the northern part of the province is that on the Upsalquitch River discovered by Dr. Ells in 1879 and described also in the 1879-80 report.

Quebec.

The Geological Survey of Canada was instituted in August, 1842, but prior to the confederation of the provinces in 1867, the scope of its operations extended only over Upper and Lower Canada, now known as the provinces of Ontario and Quebec.

With the view of ascertaining whether the coal measures of New Brunswick did or did not extend into Canada, its first director, Sir W. E. Logan, devoted the summer seasons of 1843 and 1844 to a geological examination of the Gaspé peninsula and of the country between it and the Baie des Chaleurs. In 1843 he surveyed the coast from Cap Rosier to Paspebiac, and in 1844 the exposures between Cap Rosier and Cape Chatte, thence following the Chatte River to the Cascapedia and crossing to the Baie des Chaleurs. During these two years the main geological features of the part of the province examined were, for the first time, definitely ascertained, and the absence of any productive coal measures north of the Baie des Chaleurs demonstrated. In 1843 the sandstones and limestones of Gaspé Bay, since known as the Gaspé sandstones and limestones, were carefully studied and their fossils collected. In 1844 the Gaspé sandstones were traced for a considerable distance up the St. Lawrence, and in the " Report of Progress " of the survey for 1847-48 they are said to extend from the very extremity of the Gaspé district to Matapedia Lake, a distance of 150 miles, and their thickness is estimated at 7,000 feet.

As early as 1845, if not in 1844, the Devonian age of the Gaspé sandstones was recognized by Logan. In the Annual Report of the survey under his direction for 1844 (which, though written in 1845, was not published until 1846), these sandstones are said to "resemble the Chemung and Portage groups of the State of New York, with perhaps the addition of what the geologists of that State term their old red sandstone "

(*i. e.* the Catskill group), and to be overlaid by the Carboniferous series. At that time the Gaspé sandstones were regarded as of Upper Devonian age, but the numerous fossils that Logan had collected from them had not then been critically studied by any competent palæontologist. · In an entry in his notebook for August 20, 1843, published in the " Life of Logan " by Dr. Harrington, it is distinctly stated that the plants of these sandstones are "not Carboniferous." A few years later, in a communication to the meeting of the " British Association for the Advancement of Science " at Ipswich, in 1851, Logan thus expresses himself : "None of the productive part of the New Brunswick coal measures reaches Canada, but there comes out from beneath it, on the Canadian side of the Bay Chaleurs, 3,000 feet of Carboniferous red sandstones and conglomerates. These are succeeded by 7,000 feet of Devonian sandstones, which rest upon 2,000 feet of Silurian rocks consisting of limestones and slates."[1]

Six of the species of fossil plants collected from the Gaspé sandstones by Logan in 1843 were described by Sir William Dawson : four (*Psilophyton princeps, P. robustius, Lepidodendron Gaspianum* and *Prototaxites Logani*) in the Quarterly Journal of the Geological Society of London for January, 1859 ;[2] and two (*Cordaites angustifolia* and *Selaginites formosus*) in the "Canadian Naturalist and Geologist" for June, 1861. In the former of these papers the two remarkable genera *Psilophyton* and *Prototaxites* were first proposed and defined. Subsequently, however, in 1888, Sir William somewhat modified his earlier descriptions of *Prototaxites*, and changed its generic name to *Nematophyton*.[3] *Selaginites formosus* was abandoned "as a vegetable species" by its author, in 1871, because additional material showed that the specimens upon which it was based are " probably fragments of some Eurypteroid crustacean,"[4] as suggested by Mr. Salter.

The supposed worm-tracks from the Gaspé sandstone between Tar Point and Douglastown, discovered by Logan in

[1] Report of the Twenty-first Meeting, page 61.

[2] Volume xv. p. 477.

[3] The Geological History of Plants, page 21; and Transactions of the Royal Society of Canada for 1888, sect. 4, pp. 27-47.

[4] Geological Survey of Canada. The Fossil Plants of the Devonian and Upper Silurian formations of Canada, part 1, page 65.

1843, were described and refigured by the writer, under the name *Gyrichnites Gaspensis*, in the Transactions of the Royal Society of Canada for 1882.

Logan's examinations of the Gaspé series of sandstones and limestones were supplemented by those of Murray on the Douglastown and St. John rivers in 1845, of Richardson on the Magdalen River and upper part of the Dartmouth in 1857, and of Bell on the Dartmouth, York and Malbaie rivers in 1862. Sir William Dawson, also, made extensive collections of fossils around the shores of Gaspé Bay in 1858 and 1869, and Dr. Ells a general geological survey of the Gaspé peninsula, from Gaspé Basin to the Matapedia River and from the St. Lawrence River to the Baie des Chaleurs in 1880-83, and a similar survey of the Devonian basin of the Causupscal River in 1884.

The collections made by Sir William Dawson in 1869 added thirteen additional species of fossil plants to the flora of the Gaspé sandstones, and these species were described and illustrated in the first part of his memoir on the " Fossil Plants of the Devonian and Silurian Formations of Canada," published by the Canadian survey in 1871. The "Geology of Canada," published in 1863, contains lists of some of the marine invertebrate fossils of the Gaspé limestones and sandstones, collected by Logan, Dawson and Bell, and these fossils were more fully determined or described by E. Billings in the first part of the second volume of Palæozoic Fossils, published by the Canadian survey in 1874. A small species of *Cephalaspis*, also, collected by Professor G. T. Kennedy, then one of Sir William Dawson's assistants, from the Gaspé sandstone on the north side of Gaspé Bay, in 1869, was described and figured by Professor Ray Lankester, in the Geological Magazine for September, 1870,' under the name *C. Dawsoni*.

In the "Geology of Canada" it is stated that the "limestones of Cape Gaspé appear for the most part to belong to the Lower Helderberg group. The fossils at the summit, however, bear a striking resemblance to those of the Oriskany formation, with which several of them are identical. It appears probable, therefore, that we have here a passage from the

1 **Volume VII, page 397.**

Lower Helderberg to the Oriskany, and the latter formation may be more especially represented by the lower part of the Gaspé sandstones." Eleven years later, in 1874,[1] E. Billings expressed the opinion that the lower 330 feet of the Gaspé limestones are Upper Silurian (Lower Helderberg), the middle 880 feet passage beds, and the upper 800 feet Devonian.

At the other end of the province a small area of rocks on the Famine River, in Beauce county, and another on the west side of Lake Memphremagog, in the county of Brome, were recognized as Devonian by Logan in 1863.[2]

Quite recently, too, a re-examination by Mr. Schuchert, of some of the brachiopoda from the small masses of limestone on St. Helen's Island, opposite Montreal, has shown that these limestones are probably the equivalents of part of the Hamilton formation of Ontario and New York, and not of the Lower Helderberg.

Although the Devonian system is pre-eminently the Age of Fishes, yet for many years scarcely any remains of fossil fishes had been found in the Devonian rocks of Canada, that are at all closely comparable with those of the old red sandstone of Scotland and Russia. As early as 1842, however, the rocks on both sides of the lower Restigouche River were examined by Dr. Gesner, who says that he found the "remains of fish and a small species of tortoise, with fossil foot-marks,"[3] in the shales and sandstones at Escuminac (now called Scaumenac) Bay, which he supposed were of Carboniferous age. The statement in regard to the fossils at this locality attracted no particular attention at the time, but in September, 1879, Dr. Ells found a natural mould of the exterior of the ventral surface and of one of the lateral appendages of a Pterichthys-like fish in a concretionary nodule at Scaumenac Bay, and in June, 1881, remains of a species of *Cephalaspis* in the brecciated limestones near Campbellton. The first of these discoveries led to further investigations by officers of the Canadian survey in 1880, 1881 and 1882, which revealed the existence of a remarkable assemblage of fossil fishes and land plants of Upper

1 Geological Survey of Canada. Palæozoic Fossils, vol. II, pt. I, page 1.

2 Geology of Canada, pages 428 and 436.

3 Report on the Geological Survey of the Province of New Brunswick, etc., St. John, 1843, page 64.

Devonian age at Scaumenac Bay, and of an entirely different series of fishes and plants, of Lower Devonian age, on the opposite or New Brunswick side of the river, near Campbellton. Large collections were made at each of these localities, especially of the fossil fishes, which were described by the writer in 1880,[1] 1881[2] and 1883,[3] and described and illustrated in 1887[4] and 1889.[5] Many of these specimens were exhibited and described at the meeting of this association at Montreal in 1882.

In the collections from Scaumenac Bay made up to 1882 and described by the writer, the Elasmobranchii are represented by two species of *Acanthodes* (*A. concinnus* and *A. affinis*); the Ostracodermi by numerous, remarkably well preserved and nearly perfect specimens of a *Bothriolepis* (*B. Canadensis*) which Gesner seems to have thought was a small tortoise; the Dipnoi by a supposed *Phaneropleuron* (*P. curtum*), the type of Traquair's subsequently described genus *Scaumenacia*;[6] and the Teleostomi by a *Glyptolepis* (*G. Quebecensis*), a *Cheirolepis* and a new genus (*Eusthenopteron*) closely allied to *Tristichopterus*. A few of the superficial and presumably sensory grooves on the cranial shield of the Canadian *Bothriolepis* were mistaken for sutures, as the similar ones of the European species had been by Lahusen, but some of the specimens of that genus from Scaumenac Bay threw quite a new light on the structure of its mouth organs, and of the so-called "lid" with its pineal element. And, similarly, a portion of one side of the head of a specimen of *Eusthenopteron* from the same locality, which by an oversight was referred to *Phaneropleuron*, has almost all the sclerotic plates of the eye preserved.

From the collections made near Campbellton in 1881 and 1882, four species of fossil fishes were described, *viz.*, *Cephalaspis Campbelltonensis;* a supposed *Coccosteus* (*C. Acadicus*)

[1] American Journal of Science, vol. xx, page 132 ; and reprinted in the "Canadian Naturalist and Geologist," vol. x, page 23.
[2] American Journal of Science, vol. xxi, page 94 ; and reprinted in the Annals and Magazine of Natural History. fifth series, vol. viii, page 159 ; and "Canadian Naturalist and Geologist," vol. x, new series. p. 27. and p. 93.
[3] American Naturalist, vol. xvii, p. 158.
[4] Transactions of the Royal Society of Canada, vol. iv, sect. 4, p. 101.
[5] *Ibid.*, vol. vi, sect. 4, p. 77.
[6] Geological Magazine, June, 1893. Decade 3, vol. x. p. 262.

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the type of Traquair's subsequently characterized genus *Phlyctœnaspis;*[1] and two kinds of fin spines.

Numerous fossil fishes from both of these localities have since been collected by Mr. Jex for Mr. R. F. Damon, of Weymouth, England, and these have been acquired by the Edinburgh and British museums. These later collections have yielded some additional species, one from Scaumenac Bay, which was described by Dr. Traquair in 1890; and six from near Campbellton, three of which were described by Dr. Traquair, one in 1890 and two in 1893, and three by Mr. A. Smith Woodward in 1892. The latest novelty from Scaumenac Bay is a new *Cephalaspis* (*C. laticeps* Traquair), of which it is said that "this is the first occurrence of a cephalaspid in rocks of later age than the Lower Devonian."[2] The three additional species from Campbellton, that Dr. Traquair has described, are two ichthyodorulites (*Gyracanthus incurvus*[3] and *Cheiracanthus costellatus*)[4] and another *Cephalaspis* (*C. Jexi*).[5] The three from the same locality described by Mr. A. Smith Woodward, in the eighth volume of the Third Decade of the Geological Magazine, are all elasmobranchs, *viz.*, *Acanthodes semistriatus*, *Protodus Jexi*, and *Diplodus problematicus*, the latter being the type of Traquair's genus *Doliodus*,[6] published in 1893.

In 1882 Sir William Dawson determined or described the fossil plants from Scaumenac Bay, four specifically and four only generically, and identified six species of fossil plants from near Campbellton with the *Psilophyton princeps*, *P. robustius*, *Arthrostigma gracile*, *Leptophlœum rhombicum*, *Cordaites angustifolia* and *Prototaxites Logani* of the Gaspé sandstones. He asserts that the plant and fish-bearing beds at Scaumenac Bay are "no doubt the equivalents and continuation of the upper part of the Gaspé sandstones," and that the fossil plants from near Campbellton are "perfectly identical with the lower part"[7] of these sandstones.

1 Geological Magazine, Decade III, vol. VII, page 144.

2 *Ibid.*, Decade III, vol. VII, page 16.

3 *Ibid.*, page 21.

4 *Ibid.*, Decade III, vol. X, page 146.

5 *Ibid.*, page 147.

6 *Ibid.*, page 145.

7 Geological Survey of Canada. The Fossil Plants of the Erian (Devonian) and Upper Silurian formations of Canada. Part 2.

2. ONTARIO AND KEEWATIN (HUDSON BAY).

While Logan was exploring the Gaspé sandstones, in 1843, Mr. A. Murray, then assistant geologist to the Canadian survey, was engaged in a " geological examination of the district lying in a general line between Georgian Bay, on Lake Huron, and the lower extremity of Lake Erie." In his report on that year's operations, published in 1845, Mr. Murray correctly, and for the first time, regards the rocks at Port Colborne, Cayuga, etc., which he calls the Upper Limestones, as the equivalents of the Corniferous limestone of the State of New York. The black bituminous shales at Kettle Point, Lake Huron, and on the Sydenham River, that he examined in 1848, he at first thought to be part of the Hamilton formation, but in 1855 he re-examined these shales and some of the exposures on the Sable River and in the township of Bosanquet, in company with James Hall, upon whose authority the former were decided to represent the lowest member of the Portage and Chemung group and the latter the Hamilton formation. But this statement was not published until 1857.

The discovery of the Oriskany sandstone at Cayuga would seem to have been made, or rather first recorded, by E. Billings in May, 1860. For, in the preface to his now classical paper " On the Devonian Fossils of Canada West," Mr. Billings says that the " Devonian rocks of Canada West consist of portions of the Oriskany sandstone, Schoharie grit, Onondaga limestone, Corniferous limestone, Hamilton, Portage and Chemung groups." This paper was originally published in four parts, and in the third and fourth parts, fourteen of the species of brachiopoda therein enumerated or described are said to occur in the Oriskany. The " Geology of Canada," published in 1863, contains a list of thirty species of fossils from the Ontario Oriskany, most of which, in the Museum of the Geological Survey at Ottawa, are labelled as having been collected by J. De Cew. In that publication it is stated that only the lowest of the three divisions of this formation extends into Ontario, that it occupies only a few small areas in the townships of Dunn, Oneida and Cayuga, as a "very narrow border" to the Corniferous, and that it " seldom exceeds about six feet in thickness." A "list of the fossils occurring in the Oriskany sandstone

of Maryland, New York and Ontario," by Mr. Charles Schuchert, published in 1889, in the "Eighth Annual Report of the Geologist of the State of New York," contains the names of seventy-six species from Cayuga. Most of the Ontario material from which this list was made was probably obtained from Mr. De Cew. But Mr. Schuchert, who made additional collections of the fossils of the Ontario Oriskany for the United States National Museum in 1895, says, in a recent letter to the writer, that he then saw how easy it is to mix Oriskany and Corniferous fossils while collecting, and believes that the collections made by Mr. De Cew are mixed. Mr. Schuchert thinks that near Cayuga there is a transition zone between the Oriskany and true Corniferous, and that many of the fossils recorded in the " Geology of Canada" as from the Oriskany may be from this zone. Further, he is of the opinion that it is only the uppermost portion of the Oriskany that is represented near Cayuga.

The fossils of the Corniferous formation or Upper Helderberg group of Ontario have been determined or described, either separately or together with those of the Hamilton formation, by E. Billings and Professor H. A. Nicholson, in Canadian publications ranging from 1857 to 1885. Incidentally they have been described or enumerated by James Hall in the thirty-fifth regent's report of the New York State Cabinet of Natural History, and in volumes four to eight of the Palæontology of that State, also by Dr. Carl Rominger in his " Fossil Corals" of Michigan.

Tabulating the information obtainable from these and other sources, and omitting names that have long been known to be synonyms, the number of species of fossils that have been recorded from this formation in Ontario would seem to be 258, as follows :

Corals (inclusive of Stromatoporoids)	100
Vermes	1
Polyzoa (= Bryozoa)	40
Brachiopoda	60
Pelecypoda (= Lamellibranchiata)	10
Gasteropoda	17
Cephalopoda	8
Ostracoda	1
Trilobita	17
Fishes	4
	258

In addition to these, there are in the Museum of the Canadian Survey a few fragmentary crinoids, several species of polyzoa, a few brachiopoda, pelecypoda and gasteropoda, and one pteropod (an undetermined species of *Tentaculites*) from the Corniferous of Ontario, that have yet to be studied.

From this list it would appear that corals form by far the most conspicuous feature in the fauna of the Ontario Corniferous. But, although in places this formation is mainly a large coral reef, it is obvious that quite a number of the species that have been proposed therefrom are based upon very insufficient characters. For some time past the writer's friend and colleague, Mr. L. M. Lambe, has been engaged in a much-needed revision of the Canadian palæozoic corals, and when this revision is completed, as it is hoped it soon will be, it will doubtless materially reduce the number of species from the Corniferous of the province. On the other hand, the number of species of polyzoa, brachiopoda and mollusca from that formation, in collections that have yet to be studied, will be quite largely increased.

The fossils of the Hamilton formation of Ontario have been reported on by Billings, Nicholson, Hall, and more recently by the writer, who has published two small monographs upon them. In the latter of these, published in November, 1898, 219 species are recognized and recorded as follows:

Sponges	2
Corals (inclusive of Stromatoporoids)	40
Echinodermata	16
Vermes	14
Polyzoa (= Bryozoa)	40
Brachiopoda	61
Pelecypoda (= Lamellibranchiata)	13
Gasteropoda	12
Pteropoda	3
Cephalopoda	8
Ostracoda	3
Phyllopoda	1
Trilobita	4
Fishes	2
	219

Several additional species of Fenestellidæ and Monticuliporidæ are indicated, in the Canadian survey and other collections, by mere fragments that have not yet been critically ex-

amined. From a comparison between the foregoing lists it would appear that echinodermata and vermes are more numerous in genera and species in the Hamilton formation than in the Corniferous, but eight of the fourteen species of vermes from the Hamilton formation are jaws or teeth of conodonts that are very small and difficult to find.

The black shales at Kettle Point, which are supposed to represent the Gennessee slates of the State of New York, have so far yielded only a still undetermined *Lingula*, and four species of fossil plants (*Calamites inornatus*, *Lepidodendron primævum*, macrospores of *Protosalvinia Huronense*, and a *Spirophyton*) that have been determined or described by Sir William Dawson.

The Tully limestone, the supposed representative of the Cuboides zone of the European Devonian, and the Naples beds, or Intumescens zone, of Western New York, have not yet been recognized in Ontario.

One of the results of the explorations of Dr. R. Bell, in 1871, 1875, 1877 and 1886, on behalf of the Geological Survey of Canada, was the discovery of a large area of Devonian rocks to the west and southwest of James Bay. In 1871 Dr. Bell collected a few fossils on the Albany River (which is now part of the dividing line between Ontario and the District of Keewatin) between Marten's Falls and the Forks; and in 1886 a much larger number on the same river below the Forks. Some of these fossils are from a yellowish gray limestone, and those obtained from this limestone in 1886 represent seventeen species. Twelve of these appear to be identical with Corniferous species from Ontario and New York State, and the remainder are either undeterminable or undescribed. Others are from small patches of red marl, and these fossils seem to indicate the Hamilton formation, the prevalent species being perfect and well preserved specimens of *Spirifera pennata* (Atwater), formerly known as *S. mucronata*, Conrad.

Collections of fossils, that are obviously of Devonian age, were made by Dr. Bell in 1875 and 1877 on the Moose River, and two of its larger tributaries, the Missinaibi and Mattagami. Lists of these fossils, most of which are identical with well-known Corniferous species, were published in the " Reports

of Progress of the Geological Survey of Canada" for 1875-76 and 1877-78. For many years a number of fossils from the Devonian rocks of the Albany River at Old Fort Henley and of the Moose River, collected by the late Mr. George Barnston about 1834 or 1835, have been in the Museum of the Canadian Survey, but nothing appears to have been published about them.

In Keewatin a few fossils, that are probably of Devonian age, were collected, in 1886, by Dr. R. Bell at two localities on the Attawapishkat River, and by Mr. Low from the Limestone Rapids on the Fawn branch of the Severn River. These fossils have not yet been critically studied, but among those from the last-mentioned locality there is a recognizable fragment of *Sphærospongia tesselata*, which is one of the most characteristic species of the Stringocephalus zone of the Manitoba Devonian. The existence of Devonian rocks on Southampton Island has been quite recently inferred from the fact that a few fossils from that island, lent to Dr. Bell by a missionary in 1898, are similar to those from the Attawapishkat River. Dr. Bell had previously stated that the limestone on Southampton Island is "evidently exactly the same as that of Mansfield Island."[1] If this be the case, the limestone of Mansfield Island may possibly be Devonian, rather than Cambro-Silurian as previously supposed.

3. MANITOBA AND THE NORTH-WEST TERRITORIES.

The Devonian age of the limestones on Snake Island, Lake Winnipegosis, and Manitoba Island, in the lake of that name, was asserted by E. Billings, in 1859, on the evidence of a few fossils collected therefrom in 1858. At that time Mr. Billings was under the impression that these limestones are, as he says, "most probably about the age of the Hamilton group."[2] In 1874 Dr. J. W. Spencer collected some fossils, which Mr. Billings pronounced to be also of Devonian age, from rocks on the islands and shore of Swan Lake and on the western shore of Dawson Bay, Lake Winnipegosis. Still more

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[1] Geological and Natural History Survey of Canada, Report of Progress for 1882-83-84. p 34 D.D.

[2] Hind's Report on the Assiniboine and Saskatchewan Exploring Expedition, Toronto, p. 187.

recently, an almost exhaustive geological examination of the islands, shores and immediate vicinity of lakes Manitoba and Winnipegosis was made by Mr. J. B. Tyrrell in 1888 and 1889. Assisted by Mr. D. B. Dowling, Mr. Tyrrell also made an exceptionally large collection of the fossils of the Devonian rocks of this region. This collection, which has been reported on somewhat fully by the writer in two illustrated papers published in 1891[1] and 1891,[2] was found to consist of 133 species, but about nineteen of these could not then be determined specifically. Two additional species of corals in this collection have since been determined, and an additional species of pteropoda from a collection made later has been described, making the total of identified or described species now known from these rocks to be 117, as follows:

Sponges (inclusive of Receptaculitidæ)	2
Corals (inclusive of Stromatoporoids)	17
Vermes	1
Polyzoa (= Bryozoa)	5
Brachiopoda	18
Pelecypoda	25
Gasteropoda	29
Pteropoda	2
Cephalopoda	9
Ostracoda	3
Trilobita	3
Fishes	3
	117

According to Mr. Tyrrell these fossils are exclusively from the Middle and Upper Devonian of the province, for the Lower Devonian has not yet been satisfactorily recognized in Manitoba, though it may be represented by about 100 feet of red and other shales, from which no fossils have yet been collected. In any case they are of special interest as showing certain well marked and not altogether unexpected points of resemblance to those of the English and European Devonian. For, the upper half of the Manitoba Middle Devonian, or Winnipegosan formation of Mr. Tyrrell, consists of a tough white dolomitic limestone holding numerous examples of a large *Stringocephalus* which is apparently identical with the *S. Burtini* of Defrance and of other European authors. More-

1 Transactions of the Royal Society of Canada, vol. VIII, sect. 4, p. 93.
2 Geological Survey of Canada, Contributions to Canadian Palæontology, vol. 1, pt. 4.

over, it is here associated with many fine specimens of *Sphœrospongia tesselata*, Phillips, and with fossils that cannot at present be distinguished from the following well-known European species.

Cladopora cervicornis (De Blainville).	Atrypa aspera, Schlotheim.
Spirorbis omphalodes, Goldfuss.	Pugnax pugnus (Martin).
Productella productoides (Murchison).	Paracyclas antiqua (Goldfuss).
	Murchisonia turbinata, Schlotheim.
Stropheodonta interstrialis (Phillips).	Euomphalus annulatus, Phillips.
	Loxonema priscum, Munster.
Atrypa reticularis, L.	Macrochilina subcostata (Schlotheim).

The Stringocephalus limestone of Manitoba would seem to occupy much the same stratigraphical position as that of Devonshire, Rhenish Prussia and Belgium, and its fossils show that it is probably their homotaxial equivalent.

Immediately above the Stringocephalus zone in Manitoba there are beds which may possibly represent the Cuboides zone, although *Rhynchonella*, or, as it is now called, *Hypothyris cuboides*, has not yet been found in them. The prevalent fossils in these beds are *Cyathophyllum dianthus* and *C. vermiculare*, var. *præcursor* (teste Frech); *Chonetes Logani* var. *Aurora*, *Productella subaculeata*, *Orthis striatula*, *Stropheodonta arcuata;* and *Cyrtina Hamiltonensis*, which the Rev. G. F. Whidborne has recently asserted is the same as the European *C. heteroclita.*

Regarding the fossils of the Manitoba Devonian as a whole, it is to be noted that it is not the corals, nor the polyzoa (or bryozoa), nor the brachiopoda that have as yet yielded the largest number of species (as they have in Ontario), but the gasteropoda and pelecypoda.

From the northern end of Lake Winnipegosis the Devonian rocks extend into the immediately adjacent district of Saskatchewan.

It has long been known that the eastern ranges of the Rocky Mountains in Alberta are mainly composed of Carboniferous or Devonian, or perhaps of Carboniferous *and* Devonian, limestones and shales. These rocks were examined in 1858 and 1859 by Sir James Hector, who writes as follows in regard to them.

"These limestones are of dark and light blue colour, crystalline, compact or cherty, with fossils that are either of Carboniferous or Devonian age, the principal of which are *Spirifer*, *Orthis*, *Chonetes*, *Conularia*, *Lonsdalia*, *Cyathophyllum*, *Lithostrotion*, *&c.*".... "Along with them are softer beds of gritty, sandy shale, generally of a dull red or purple colour."[1].... "In the second range we have the same limestones and shales repeated as in the first, but at the base I observed traces of a magnesian limestone of a buff colour, containing *Atrypa reticularis*, a true Devonian fossil."[2].... "On the Kicking Horse River, in the third range, we have the mountains again formed of blue limestone, along with a compact blue schist with red bands, giving a curious striped aspect to the rocks."[3]

"In reference to these remarks, Dr. G. M. Dawson, who made a geological examination of the South Kootanie Pass and its vicinity, in 1874, adds the following comments :

"Dr. Hector is not very clear as to the separation of the supposed Devonian and Carboniferous limestones, and they may indeed very probably belong to a single series." "Prof. Meek, in describing fossils from limestones occurring in the mountains south of the boundary line, which, from the general facies, he believed to be Carboniferous, mentions the fact that the forms, without exception, belong to genera which are common both to that formation and the Devonian, and of which a small number are represented in the Silurian."[4]

In 1881, 1883 and 1884 Dr. Dawson was engaged in an examination of the geological structure of parts of the Rocky Mountains in Alberta between Lat. 49° and Lat. 51° 30', the results of which were published in the "Annual Report of the Geological Survey of Canada" for 1885 (Vol. I, New Series). This report contains preliminary lists of a few supposed Devonian fossils, from the limestones on the summit of the North Kootanie Pass, on Crow Nest Lake, and from the lowest beds exposed at the west end of the cañon on the Cañon branch of the Elbow River.

Subsequently Mr. R. G. McConnell made a geological survey of the Rocky Mountains between the Canadian Pacific Railway and the North Saskatchewan in 1885, and a more detailed exploration than had yet been made, of the geology of those in the more immediate neighborhood of that railway, in 1886.

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[1] Palliser's Explorations in British North America, 1863, p. 239.

[2] Quarterly Journal of the Geological Society of London, vol. xvii (1861), p. 443.

[3] Palliser's Explorations in British America, p. 239.

[4] "Report on the Geology and Resources of the Region in the Vicinity of the Forty-ninth Parallel," etc., 1875, p. 71.

He published, in the "Annual Report of the Geological Survey of Canada" for 1886, a geological section across the Rocky Mountains in the vicinity of the Canadian Pacific Railway, with a diagram showing the formations represented in the sections to the west of the Castle Mountain Range, and another of those represented in sections to the east of that range. In the latter, only four geological systems or formations are recognized, namely the Cambrian, which Mr. McConnell calls also the "Castle Mountain Group;" the Devonian, which he designates also as the "Intermediate Limestone;" the Devono-Carboniferous, which he calls the "Banff Limestone;" and the Cretaceous. In the text it is stated that the Intermediate Limestone is "mainly composed of a great series of brownish dolomitic limestones and has a thickness of about 1500 feet." Its fossils are "usually badly preserved and consist mainly of almost structureless corals." The few that were collected, it may be added, have not yet been determined and indeed are scarcely determinable. According to Mr. McConnell, the Banff Limestone is the "principal constituent of all the longitudinal ranges east of Castle Mountain." It "has a total thickness of about 5100 feet and is divisible into a lower and upper limestone and into lower and upper shales." Its fossils are better preserved than those of the Intermediate Limestone, and fairly large and representative collections of the former were made.

These collections have not yet been at all exhaustively studied, but most of the species represented in them are apparently of Carboniferous age. Among those collected in 1886 are two or three small species of *Productus*; a large *Syringothyris*; a *Pugnax* closely allied to if not identical with *P. Rockymontana*, Marcou; a *Hustedia* like *H. Mormoni* (Marcou); and two well-marked pygidia of *Proetus peroccidens*, Hall and Whitfield. The specimens from the black fissile shales of the Bow River, collected by Mr. McConnell in 1885, that were provisionally referred to the Devonian genus *Clymenia* on page 18 D of his report, do not show clear indications of either septa or siphuncle, and may, therefore, be casts of a discoidal gasteropod. On the other hand, in 1885 Mr. McConnell obtained a few specimens, that are unquestionably referable to *Atrypa reticularis*, from the Rocky Mountains

at the Pipestone Pass Falls, and from the first range on the North Saskatchewan. It was from the mountains at the source of the North Saskatchewan that the specimens were collected by Sir James Hector which Salter referred to *A. reticularis*.

In 1898 Mr. J. McEvoy collected a few fossils at several localities in the first foot-hill of the Rocky Mountains, in Alberta, where it intersects the valley of the Athabasca. These fossils have not yet been very critically examined, but those from two of these localities are probably Carboniferous, and the remainder either Carboniferous or Devonian.

In 1868 Mr. F. B. Meek published a paper entitled "Remarks on the Geology of the Valley of Mackenzie River, with figures and descriptions of Fossils from that region, in the Museum of the Smithsonian Institution, chiefly collected by the late Robert Kennicott, Esq.," in the first volume of the Transactions of the Chicago Academy of Sciences. The paper consists of a concise history of the discovery of Devonian rocks at various localities in the Athabasca, Mackenzie River and Yukon districts, by Sir John Franklin, Sir John Richardson, Mr. A. K. Isbister, Major R. Kennicott, Mr. R. W. McFarlane, Mr. B. R. Ross and the Rev. W. W. Kirby, followed by descriptions or identifications of thirty-two species of Devonian fossils. Of these species ten are corals, twenty-one are brachiopoda and the remaining one is a cephalopod. Mr. Meek expresses the opinion that the Devonian rocks exposed on the Clearwater, Athabasca, Slave, and Mackenzie rivers, and on Great Slave Lake, are probably referable to the Hamilton formation.

Since 1868 Devonian rocks have been discovered or examined by officers of the Geological Survey of Canada, and their fossils collected, at the following localities in this region. In the Athabasca district, at four different exposures on the Athabasca River and at one each on its tributaries, the Clearwater, Red and Pembina rivers, by Professor Macoun in 1875, by A. S. Cochrane in 1881, by Dr. R. Bell in 1882 and by R. G. McConnell in 1890; also at three different exposures on the Peace River by Professor Macoun in 1875 and by Mr. McConnell in 1879. In the Mackenzie District, on the banks of

the Long Reach of the Lower Liard River and on the Hay River forty miles above its mouth by Mr. McConnell in 1887, and at four different and rather widely distant exposures on the Mackenzie River by Mr. McConnell in 1888.

Most of the fossils from these localities that were collected before 1875 have been provisionally reported on in the Reports of Progress of the Canadian Survey for the years in which they were made. Those, however, that were collected between the years 1875 and 1890, both inclusive, form the subject of an illustrated paper, by the writer, on " The Fossils of the Devonian Rocks of the Mackenzie River Basin," published in 1891.[1] This publication, which is practically a continuation of Mr. Meek's paper on the same subject, already referred to, adds fifty-seven additional species of purely marine invertebrata to the previously known fauna of these rocks, as under :

Spongiæ	1
Corals (inclusive of Stromatoporoids)	10
Crinoidea	1
Vermes	3
Polyzoa (= Bryozoa)	7
Brachiopoda	20
Pelecypoda	7
Gasteropoda	3
Pteropoda	1
Ostracoda	3
Trilobita	1
Total	57

According to Mr. McConnell, a section of the Devonian rocks in the Mackenzie district, in descending order would be somewhat as follows :

1. Upper limestone	(about)	300 feet
2. Greenish and bluish shales alternating with limestone	(about)	500 feet
3. Grayish limestone, interstratified with dolomites, the lower part of which may be older than the Devonian		2,000 feet (or more.)

The whole of the fossils collected by Mr. McConnell, Professor Macoun and Dr. Bell are from the upper part of the middle division of this section. Of the fifty-seven species of fossils in the foregoing list, twenty-two are apparently found also in the Hamilton formation of Ontario and the State of New York ; ten (but only six additional ones) in the Devo-

[1] Geological Survey of Canada, Contributions to Canadian Palæontology, vol. 1, part 3.

nian rocks of Iowa now referred to the Chemung ; and seven in
the Chemung of the states of New York and Pennsylvania.
On the other hand, there are strong reasons for supposing that
the whole of these fossils are from a horizon nearly correspond-
ing to that of the "Cuboides zone" of Europe. In the first
place, three specimens of a brachiopod which the writer has
identified with the *Rhynchonella* (now called *Hypothyris*), *cu-
boides* of Sowerby, were collected by Mr. McConnell, one at
the Hay River in 1887, and two on the Peace River at Ver-
milion Falls in 1889. It is true that Mr. Schuchert thinks
that these three specimens should be called *Hypothyris Em-
monsi*, but Mr. Walcott had previously expressed the opinion
(in 1884) that "there is little doubt but that *Rhynchonella in-
termedia, R. Emmonsi* and *R. venustula*, Hall, are varieties of
R. cuboides,[1] of the Devonian of Europe." On the Hay and
Peace rivers the supposed *Hypothyris cuboides* is associated
with *Spirifera disjuncta* (or *Verneuili*), and other fossils that
are elsewhere supposed to be characteristic of the Cuboides
zone are to be met with in the published lists of species from
the Athabasca and its tributaries, or ,the Mackenzie. The
discovery by Mr. McConnell at the Ramparts on the Macken-
zie River, of two large specimens of a Stringocephalus which
cannot at present be distinguished from *S. Burtini* may indi-
cate a northwestward extension of the Stringocephalus lime-
stone of Manitoba. The still later recognition by Dr. John
M. Clarke, in 1898, of *Manticoceras intumescens* in the cast of
the interior of three chambers of the septate portion of a species
of Goniatite from the Hay River, collected by Mr. McConnell
and figured by the writer, would seem to indicate the exist-
ence of the equivalent of the "Intumescens zone" or Naples
fauna at that locality.

The present state of our knowledge of the Devonian rocks
of the whole Dominion, from a purely palæontological stand-
point, may be thus briefly summarized. We now possess a
fairly satisfactory knowledge of the fossils of the Devonian
rocks of Ontario, and of the relations which these rocks bear
to the typical section in the State of New York. The fossil

[1] Monographs of the United States Geological Survey, vol. viii, (Palæontology
of the Eureka District), page 157.

plants of the Gaspé sandstones have been described and figured by Sir William Dawson, and the remarkable assemblages of fossil fishes from the Upper Devonian of Scaumenac Bay and Lower Devonian near Campbellton have been worked out somewhat exhaustively, the earlier collections in Canada, and the later ones by the best ichthyological authorities in London and Edinburgh.   We have now some idea of the fossil fauna of the Manitoba Devonian, and have added materially to our knowledge of the fossils of the Devonian rocks of the Athabasca and Mackenzie River districts.   But, on the other hand, our knowledge of the organic remains of the Devonian of Nova Scotia is still in its infancy, and it would seem that the plant-bearing beds near St. John, N. B., which have so long been regarded as Devonian, may possibly be Carboniferous.   In the Rocky Mountain region of Alberta we have not always succeeded in distinguishing Devonian rocks from Carboniferous, and we have yet to obtain a much fuller knowledge than we now possess of the Devonian fossils of Keewatin and the area to the southwest of James Bay.

Ottawa, June 28, 1899.

PAPERS READ.

[ABSTRACTS AND TITLES.]

THE GEOLOGY OF COLUMBUS AND VICINITY. BY EDWARD ORTON, Columbus, Ohio.

GLACIAL PHENOMENA OF CENTRAL OHIO. BY FRANK LEVERETT, Denmark, Ohio.

LATERAL EROSION AT THE MOUTH OF THE NIAGARA GORGE. BY G. FREDERICK WRIGHT, Oberlin, Ohio.

AGE AND DEVELOPMENT OF THE CINCINNATI ANTICLINE. BY AUGUST F. FOERSTE, Dayton, Ohio.

THE SILURIAN-DEVONIAN BOUNDARY IN NORTH AMERICA. BY HENRY S. WILLIAMS, New Haven, Conn.

THE SECTION AT SCHOHARIE, N. Y. BY JOHN J. STEVENSON, New York, N. Y.

THE GEOLOGICAL RESULTS OF THE INDIANA COAL SURVEY. BY GEO. H. ASHLEY, Indianapolis, Ind.

THE CAPE FEAR SECTION IN THE COASTAL PLAIN. BY J. A. HOLMES, Chapel Hill, N. C.

Tuesday, August 22, joint session with Geol. Soc Am.

TRIASSIC COAL AND COKE OF SONORA, MEXICO. BY E. T. DUMBLE, Houston, Tex.

SOME GEOLOGIC CONDITIONS FAVORING WATER-POWER DEVELOPMENTS IN THE SOUTH ATLANTIC REGION. BY J. A. HOLMES, Chapel Hill, N. C.

The "fall-line," between the coastal plain region and Piedmont plateau region or hill country is the most clearly defined geologic and topographic line or zone in the eastern United States; and it is here that we find the geologic conditions most favorable for water-power developments. In the region of the crystalline schist the lesser geologic boundary lines separating belts of slate, schist, granites, etc., shearing lines or zones, and fault lines, supply favorable conditions on a smaller scale. Within certain areas of bedded or schistose rocks, whether highly tilted or horizontal, variations in the composition and obduracy of the rock masses furnish suitable conditions.

(*a*) PAROPSONEMA: A PECULIAR ECHINODERM FROM THE INTUMESCENS FAUNA, NEW YORK. (*b*) REMARKABLE OCCURRENCE OF ORTHOCEROS IN THE ONEONTA SANDSTONES OF NEW YORK. (*c*) THE SQUAW ISLAND "WATER BISCUIT," CANANDAIGUA LAKE, NEW YORK. BY JOHN M. CLARKE, Albany, N. Y.

(*a*) An extraordinary type of echinoderm structure, believed to be an echinoid, briefly described. Illustrated with drawings.

(*b*) A stratum of sandstone extending over several square miles in the Chenango Valley, N. Y., contains thousands of Orthocerata standing erect in the sediment, being the only truly marine organism in the strata. (Specimens.)

(*c*) An interesting illustration of the effect produced by aquatic vegetation upon the precipitation of lime salts. (Specimens.)

THE POT HOLES OF FOSTER'S FLATS (now called Niagara Glen) ON THE NIAGARA RIVER. BY MISS MARY A. FLEMING, Buffalo, N. Y.

I have not seen any record of the pot holes along the Niagara River in Foster's Flats. Many references have been made to the terrace, in articles relating to glaciation or to the age of the Falls, but the pot holes have been overlooked or were not considered of sufficient importance to merit study.

Wednesday, August 23.

In 1898 the pot holes were easily found along the edge of the river for half a mile or more toward the whirlpool. Now one must seek further as work in progress there is fast obliterating them. They occur in masses of rock which have been broken off and tumbled down from the adjacent limestone cliffs, and it is more than probable that they were formed while the rock was in situ and before the masses were detached, although some of these masses, lying in the water or near it, show plainly that small holes (some with pebbles in them) have been formed while the rocks were lying in their present positions.

A CONSIDERATION OF THE INTERPRETATION OF UNUSUAL EVENTS IN GEOLOGIC RECORDS, ILLUSTRATED BY RECENT EXAMPLES. BY FREDERICK W. SIMONDS, Austin, Tex.

An examination of many geologic reports cannot fail to show that, as a rule, the working geologist devotes but little time to the interpretation of events, though the most valuable data may be furnished by the very rocks which, as a stratigrapher, he places on his map or records in his note-book. While none, of course, dispute the value of stratigraphic work, the value of interpretation should not be underestimated, for by it we gain our clearest insight into the physical conditions of the past. In this paper it is the purpose of the author to show that under some conditions, especially when unusual events have been recorded, that there is not only difficulty in making the proper interpretation, but even danger that the interpretation may become misleading—a partial truth being, under some circumstances, conducive to positive error. A number of recent events, of an unusual kind, are cited, for the purpose of illustrating the fact that had they occurred in the past, and had their geologic record been translated in the ordinary manner, the interpretation would have been incorrect and the resulting picture unreal.

Friday, August 25.

THE PRE-LAFAYETTE (TENNESSEAN) BASE-LEVEL. BY W J McGEE, Washington, D. C.

The most extensive base-level of the North American continent is that preserved in part as an unconformity beneath the Lafayette formation, and in part as a somewhat dissected surface extending inland from the margin of the formation. This base-level is the record of a vast period of approximate continental stability, which has been called Tennessean.

THE RELATIVE AGES OF THE MAUMEE GLACIAL LAKE AND THE NIAGARA GORGE. BY CHAS. E. SLOCUM, Defiance, Ohio.

The time thought necessary for the wearing of the Niagara Gorge to its

present extent has been shortened from time to time by different geologists until now the comparatively short period of 7,500 or 7,000 years is in favor. Granting that the methods for reaching these deductions are valid, it appears desirable to emphasize the fact that this gorge marks but one era or period in dating back to the ice age; in fact the ice age was probably well over before the waters of the great lakes began to wear this gorge. The level of Lake Erie has been little, if any, lowered by this gorge. It has been lowered by the Niagara River only as much as the channel at its origin, the foot of the lake, shows, which is not great.

The Maumee Glacial Lake was well drained before the Niagara River channel was worn, and the ice must have disappeared from the Lake Erie region previous to this,—that is to say that the Maumee Glacial Lake may have existed several thousand years before the wearing of the Niagara Gorge began.

The Galt Moraine and Associated Drainage. By F. B. Taylor, Fort Wayne, Ind.

He describes glacial features of an area in Canada, west of Lake Ontario. About 50 miles of a moraine, called the Galt Moraine, extending northeast from Paris, past Galt to Credit Forks, is shown and fragments of two others, one to the west of it and the other to the east. In the northern part of its course the Galt Moraine is on, or close to the edge of the great escarpment which runs northward from Hamilton at the west end of Lake Ontario. A large river carried the glacial waters toward the southwest along the front of the moraine in the early stages of its formation. The bed of this river is well marked throughout. As the ice front slowly drew back during the deposition of the moraine, the river changed its course and for a time ran between the ice and the back of the moraine. For several miles its bed is on the brink of the escarpment. Along that part there is no bank on the east side of the bed, but a descent of over 200 feet to the Credit River. Farther down, the river turned to the west, cut a deep bed through the moraine, and joined its earlier bed at Eden Mills. In the earlier stages of the moraine the river took a southwest course from Preston past Ayr, but in the last stages a lower course was found through the moraine, longitudinally past Galt and Paris. In its narrowest parts the old river bed is nearly a quarter of a mile wide and 55 feet deep. In most of its course the flow was rapid enough to carry away the finer drift and leave the limestone ledges bare. In two expanded portions it deposited large masses of rounded cobble stones and extensive gravel beds. The northern part of the area is well covered with drumlins. Further work will be required to show the relation of these moraines to those of Western New York and the west side of the Ontario peninsula.

GLACIAL AND MODIFIED DRIFT IN MINNEAPOLIS, MINN. BY WARREN UPHAM, St. Paul, Minn.

Red drift brought from the Lake Superior region is here overlapped by gray and bluish drift that came partly from the Red River Valley and Manitoba. The final melting of the ice-sheet laid bare an area occupied by the glacial Lake Hamline, close east of Minneapolis, between tracts of ice thus flowing from the northeast and northwest. In the eastern part of Minneapolis a terminal moraine chiefly consisting of northeastern drift was formed on the border of the western ice tract. It is evident that the glacial current from the west pushed back that from the east near the end of the ice age. The sand plain of the Mississippi Valley here was deposited near the front of the ice when it retreated westward from this moraine, and a wide esker ridge, two miles long, formed at the same time, lies in the southwest part of the city. Frequent interbanding and irregular mingling of the red and gray indicated that they were englacial, the reversed upper and central currents of the confluent ice-fields having swept the gray drift forward over the lagging red drift near the ground.

THE OZARKIAN AND ITS SIGNIFICANCE IN THEORETIC GEOLOGY. BY JOSEPH LE CONTE, Berkeley, Cal.

The existence of a long and important epoch directly preceding the invasion of the ice sheet is recognized under the name of Ozarkian by Hershey, to commemorate the erosive work of the epoch in the Ozark Mountain region.

Accepting the threefold division of the Quarternary by Upham, the Ozarkian is characterized by elevation and erosion, the Glacial by ice accumulation and drift deposits, the Champlain by depression and stratified deposition. Characteristic phenomena accompanying each of these periods in (a) the eastern part of the continent, in (b) the midcontinental or plateau region, and in (c) the Sierra region.

In discussing critical periods of the earth's history I have tried to show that there have been certain times of great and widespread changes in the earth's crust, in its climate, and in its organic forms. These critical periods separate the primary divisions or eras. The last of these was the Quarternary. An era is a great cycle of widespread changes (the Palæozoic, the Mesozoic, the Cenozoic) and such another I am convinced is now commencing and I have called it the Psychozoic.

The Quarternary is the transition critical period between the Cenozoic and the Psychozoic, when man was introduced. He became *established* as the dominant factor in the Psychozoic Era.

THE DISCOVERY OF NEW INVERTEBRATES IN THE DINOSAUR BEDS OF WYOMING. BY E. H. BARBOUR AND W. C. KNIGHT, Lincoln, Neb.

The recent discovery of some eight or ten new invertebrates in the di-

nosaur beds; all apparently fresh-water forms, will assist in confirming
belief in the fresh-water nature of these beds. The writers noted three or
four Lamelibhranchs and at least as many distinct Gasteropods. This num-
ber will be increased perhaps when the specimens, which are well pre-
served, are worked out of the matrix and studied; associated with the in-
vertebrates are crocodillian teeth and bones.

THE RAPID DECLINE OF GEYSER PHENOMENA IN THE YELLOWSTONE
NATIONAL PARK. BY E. H. BARBOUR, Lincoln, Neb.

The rapid decline of geyser phenomena in the Yellowstone National
Park seems to be little understood, but to those who visit this spot fre-
quently, the changes seem startling, and to the geologist even alarmingly
rapid. If one may judge from impressions, it seems safe to assume that
if the decline in geyser activity noted during the past four years should
continue for the coming eight or ten years, the features which most im-
press the geologist will have disappeared. As a warning, every geologist
who intends visiting the park should not postpone the trip a year, but
should visit it at once ! It may be stated generally that this decline of
activity is manifest in the geysers, mud geysers, and paint-pots, pools,
and steam vents, many of which have become wholly or partly extinct in
the past four years. As specific cases, it may be stated that at the Mam-
moth Hot Springs the activity seems not one-tenth that of former times,
Minerva terrace having become extinct (since 1895), the discharge from
Pulpit and Jupiter terraces having greatly declined during the same time,
and the Narrow Gauge—a fissure-vent—and other attractions becoming all
but extinct. Roaring Mountain is now silent though steaming. In the
Norris Geyser Basin the Black Growler is less active. In the Lamer Basin
the Splendid Fountain Geyser is extinct, with a feeble substitute near by,
named the Dewey. The Giant Paint-pots are greatly contracted in size,
the pink half being extinct, in the Upper Basin. Some of the better
known as well as many of the lesser geysers are extinct or supposed to be.
Among these are the Splendid Geyser and the Bee Hive. The Grand
Geyser which used to play daily now erupts irregularly about three times
a season. The Cascade, which erupted about every quarter of an hour in
1895 now plays once a day. The unmistakeable impression of frequenters
of the park is that the changes are serious and much more rapid than is
generally believed.

GREATEST AREA AND THICKNESS OF THE NORTH AMERICAN ICE SHEET.
BY WARREN UPHAM, St. Paul, Minnesota.

Evidence is presented, from the overlapping and intermingling of drift
deposits, that the ice-sheet at its culmination reached continuously across

the continent, from New England, Newfoundland, and Labrador, to British Columbia and southeastern Alaska, interrupted only in its southern part by the projecting ranges of the Rocky Mountains. This view differs from the conclusions of Dr. G. M. Dawson, who thinks that the Cordilleran glaciation mainly preceded the glaciation of the Laurentide region and of the great plains stretching west nearly to the Rocky mountains. His opinion that the maximum extension of the Laurentide ice-sheet was attended by a depression of the Cordilleran region with marine submergence there to a level that has since been elevated about 5,300 feet, a vertical Late Glacial and Postglacial uplift of one mile, is regarded in this paper as less acceptable than the opinion here advanced, that the Cordilleran and Laurentide ice-sheets, having been each accumulated because of high continental altitude much exceeding that of the present time, were confluent along the east side of the Rocky Mountains, a single ice-sheet at the north being continuous from the east to the west side of the continent.

In Minnesota and North Dakota, observations on each side of the glacial lake Agassiz oppose the view of Tyrrell, that the Laurentide or Labradorian ice-sheet was preceded by a Keewatin ice-sheet. The glacial lake deltas demonstrate contemporaneous glaciation meeting there from the northeast and northwest. It is also known, by overlapping drift formations, that in the east central part of Minnesota, from the latitude of St. Cloud and Pine City southward to Minneapolis and St. Paul, the northwestern ice-field, belonging to the Keewatin sheet of Tyrrell, pushed back the northeastern ice-field, referable to his Laurentide sheet, showing that there the greatest Keewatin extension was the later. The ice-fields there, however, as stated in my previous paper presented before the Section at this meeting, were contemporaneous and confluent from the northeast and northwest.

It is shown by the height of glaciation in mountain regions, and by the transportation of the glacial drift, that the ice-sheet attained a maximum central thickness of about one to two miles, nearly across the continent, this being greatest upon the Laurentide highlands.

SECTION F.

ZOOLOGY.

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ADDRESS

BY

SIMON HENRY GAGE

VICE-PRESIDENT AND CHAIRMAN OF SECTION F.

THE IMPORTANCE AND THE PROMISE IN THE STUDY OF THE DOMESTIC ANIMALS.

It is believed that for the advancement of science, no better service can be rendered by those of considerable experience as teachers and investigators than to point out to their younger brethren lines of study and research which are, on the one hand, important, and on the other promising of results. I have, therefore, selected for the subject of this address before the section of zoology a plea for the study of the domestic animals. The young zoologist may rightfully ask the grounds for studying this heterogeneous, greatly modified series of animals. In the first place it must be confessed that for the animal kingdom as a whole it applies mainly to a single one of the twelve phyla in the animal series given by Parker and Haswell—that is, to the vertebrates. The other eleven phyla —that is, the whole of the invertebrates except the arthropoda—are ignored. I wish to express very clearly and emphatically at the outset that the plea will not be made because the domestic animals seem to me alone worthy of study by zoologists, or that they are in all cases the best possible representatives of their group. It is most earnestly believed, however, that in the whole range of zoology no forms offer a greater reward for

the study of the problems of life, especially in the higher groups, than the domestic animals. The importance of the study cannot be overestimated from a purely scientific standpoint, and certainly if the prosperity, happiness, and advancement of the human race are put in the count the subject is of transcendent importance.

A glance at the tabular arrangement of the domestic animals will show where they are situated in the animal kingdom. In the great group of Invertebrates the two domesticated species —the Honey-bee and the Silkworm—may be properly compared to minute islands in a great ocean. Among the Vertebrates, on the other hand, the domestic forms are represented in two of the six classes; *viz.*, in the Birds and Mammals, and where represented are among the most prominent and important members of the various orders:

DOMESTIC INVERTEBRATES.

The Honey-bee (*Apis mellifica*).
The Silkworm (*Bombyx mori*).

DOMESTIC VERTEBRATES.

Class Aves—Birds.

1. **Natatores** { Goose (*Anser cinereus*).
   Duck (*Anas buschas*).
   Swan (*Cygnus gibbus*).

2. **GRALLATORES**—Waders (no domestic forms).

3. **Gallinaceæ** { Hen (*Gallus domesticus*).
   Turkey (*Meleagris americana*).
   Peacock (*Pavo cristatus*).

4. **Columbinæ**—Pigeon (*Columba livia*).

5. **SCANSORES**—Climbers (Parrots, woodpeckers, etc.). (No domestic forms.)

6. **Passeres**—Canary Bird (*Serinus canarius*).

7. **RAPTORES**—(Formerly the Falcon was in a sense domesticated).

8. **Cursores**—Ostrich (*Struthio camelus*).

Class—Mammalia.

(A) **MONOTREMATA** (Forms which lay eggs).

(B) **MARSUPIALIA** (Forms without true placenta).

ORDERS OF PLACENTAL MAMMALS.

1. **EDENTATA**—Armadillo, sloth, etc.
2. **CETACEA**—Whale, porpoise, etc.
3. **SIRENIA**—Manatee and Dugong.

4. **Ungulata**
- Horse (*Equus caballus*).
- Ass (*Equus asinus*).
- Pig (*Sus scrofa*).
- Camels { *Camelus dromedarius.* / *Camelus bactrianus.* }
- Sheep (*Ovis aries*).
- Goat (*Capra hircus*).
- Ox (*Bos taurus*).
- Elephant (*Elephas indicus*).

5. **Carnivora**
- Cat (*Felis domestica*).
- Dog (*Canis familiaris*).

6. **Rodentia**
- Rabbit (*Lepus cuniculus*).
- Guinea Pig (*Cavia cobaya*).

7. **INSECTIVORA**—Mole, hedgehog, etc.

8. **CHEIROPTERA**—Bats.

9. **PRIMATES**
- Monkeys and apes.
- Man (*Homo sapiens*).

NOTE—In the table of birds the ordinal arrangement is that of Claus. It will be noted that five of the eight orders of birds have domesticated representatives. Among placental mammals three of the nine orders are represented, the order Ungulata containing the larger number and the most important representatives. A few forms in addition to those named in the tables have been, at some time, more or less completely domesticated.

With the personnel of the subject for discussion thus fairly before us, what has been, what is, and what is likely to be the influence of these forms in the rise and progress of knowledge in the broad field of zoology? Or more specifically, (1) What has been and what is likely to be the influence of the study of the domestic animals upon the doctrine of the evolution of organic forms? (2) What has the study of them contributed in comparative anatomy, embryology, and physiology? (3) What has been the contribution in hygiene and preventive medicine? (4) And, finally, what should be their influence in theories of heredity and sociology?

If we would realize the value of the Doctrine of Evolution, let us imagine for an instant that this doctrine of 'orderly change' were eliminated from the knowledge of men!

To turn the zoologist back to the old notions of special and independent creation for each species or group of animals, would be like leaving the astronomer only the sun-god, and the angels to direct the planetary movements. No, it is now next to impossible to conceive of zoology struggling to comprehend the animal kingdom without this guiding principle. If similarity of form, color, structure, and stages in embryology, func-

tion and even diseases are mere coincidences without further meaning, then, indeed, from a scientific standpoint, one might as well spend his life and thought on Chinese puzzles as upon zoology. But that there is meaning and inspiration in the study of zoology requires no argument from me in this company, for on the altars of this section still burns the sacred fire kindled by our absent members—Agassiz, Leidy, Cope, Allen, Marsh and a host of others who now 'see as they are seen, and know as they are known.' It is only for me to endeavor to point out some ways in which that study may be most productive.

If the doctrine of evolution has so illuminated the way, given to the work meaning and point which it never had before, it is pertinent to ask, to what are we indebted for the general belief in this doctrine? No better answer can be given than in the words of Darwin himself in the introduction to the 'Origin of Species:' "It is, therefore, of the highest importance to gain a clear insight into the means of modification and coadaptation. At the commencement of my observations it seemed to me probable that a careful study of domesticated animals and cultivated plants would offer the best chance of making out this obscure problem. Nor have I been disappointed; in this and in all other perplexing cases I have invariably found that our knowledge, imperfect though it be, of variation under domestication, afforded the best and safest clue. I may venture to express my conviction of the high value of such studies, although they have been very commonly neglected by naturalists." In a work published on this side of the Atlantic, the author, Professor L. H. Bailey, a member of our old Section of Biology, boldly faces those who, still doubting, say: "perform this miracle of changing one species into another before our eyes, and we will believe;" and says: "If species are not original entities in nature, then it is useless to quarrel over the origination of them by means of experiment. All we want to know, as a proof of evolution, is whether plants and animals can be profoundly modified by different conditions, and if these modifications tend to persist. Every man before me knows, as a matter of common observation and practice, that this is true of plants. He knows that

varieties with the most marked features are passing before him like a panorama. He knows that nearly every plant which has been long cultivated has become so profoundly and irrevocably modified that people are disputing as to what wild species it came from. Consider that we cannot certainly identify the original species of the apple, peach, plum, cherry, orange, lemon, wine-grape, sweet potato, Indian corn, melon, bean, pumpkin, wheat, chrysanthemum, and nearly or quite a hundred other common cultivated plants. It is immaterial whether they are called species or varieties. They are new forms. Some of them are so distinct that they have been made the types of genera. Here is an experiment to prove that evolution is true, worked out upon a scale and with a definiteness of detail which the boldest experimenter could not hope to attain, were he to live a thousand years. The horticulturist is one of the very few men whose distinct business and profession is evolution. He, of all other men, has the experimental proof that species come and go." * * * Almost or quite as strong a statement might be made concerning domestic animals, as stock breeders and fanciers well known. But the more cautious may say, and have said : "This is the work of man's hands ; man who ate of the forbidden tree and became like unto the gods, a lesser creator." Well, here again, ages before coming under man's dominion one of the domestic forms gave the final demonstration.

Those who have read the masterly argument of Huxley in the American addresses on evolution, the address of Marsh before the old Natural History Section of this Association in 1877, and the address of Osborn before this section in 1893, know well the story. Starting with the generalized, five-toed forms of the basal Eocene in our own country, passing through many modifications and lateral experiments as they may be called, the five-toed form gradually became the four-, three-, and finally the one-toed modern horse, with its allies, the ass, and the zebra. Thus long before the Coast Range was brought forth, while still the eternal hills were young, the primitive horses disported themselves in vast multitudes in our Western Territories ; and it is believed that from this continent they passed to Asia, Africa, and Europe, only to come back in these

latter days to this so-called New World after making the circuit of the entire earth.

Among living forms perhaps no creature aided more in carrying conviction to the mind of Darwin himself, and to countless other people, than the common domestic pigeon. For most of the domestic animals it is usual to bring in a hypothetical, fossil type, so widely have the living forms departed from any living wild types, and so true to the domestic types do the offspring hold ; but with the pigeon it is not uncommon that reversions to the parent form occur, and even in the most modified forms reversions occur, so that there is substantial agreement that the parent stock is the wild rock pigeon (*Columba livia*). That there should be reversions in some forms is astounding, for even the number of vertebræ has become changed by domestication.

If, then, this study of domesticated and cultivated forms has thrown so much light upon this great subject of evolution as the method of nature, is there not promise of rich return for future study? And there is need of future study, for only a beginning has yet been made in this great field.

Let us now turn from Evolution to discuss for a few moments the help which the domestic forms have given to Anatomy, Embryology, and Physiology.

If one asks of what animals the structure is known in the greatest detail it must undoubtedly be answered that the structure of man has been most thoroughly explored; then come the domestic animals, especially the horse, dog, cat, and rabbit. Much of this work was done before the doctrine of evolution illuminated the way and gave meaning to rudiments or vestiges and to homologies. Still it must be said, in truth, that the older zoologists, with a rare insight, discussed large questions of homology, and recognized at bottom the real relationship of many different forms. It was, however, only the philosophical and far-sighted few who did so. The majority of anatomical work was done for its purely practical bearing on medicine and surgery. It thus happened that human anatomy exerted a powerful influence, indeed, so powerful that names were carried over into the invertebrates, for parts which could hardly, by the greatest stretch of imagination, be homologous with the

structures in man from which they were named. If there was any relationship it was of function or analogy rather than that fundamental kinship expressed by homology. Thus the legs of a horse and a spider are for the same general purpose. They are analogous, not homologous organs. Therefore, in many cases in the older morphological work one should not be deceived by supposing that there was any real insight into the phylogenetic relationship of the two forms whose parts were similarly named. While there has been a great tendency to designate parts alike which have only a fancied or analogous relationship, there has been a more harmful tendency to ignore real relationships. Only purely practical ends have been too often in view, and the real kinship of forms as little known as cared about.

What is urgently needed at the present time in comparative anatomy, especially that relating to the domestic animals, is a thorough revision in the light of this last half of the nineteenth century; then the student, whether especially trained in human, veterinary, or comparative anatomy, could pass from form to form and far more easily correlate truly homologous parts, because they would bear the same designations; and he would thus be led to see and appreciate the true kinship, although at first sight there might appear to be only unlikeness.

If any one cannot see the force of what has been said, or does not feel any lack in the present conditions, let him think of the different joints in the limbs of man, horse, dog, chicken, and honey-bee; or let him ask some one who knows the animals well, but is untrained in advanced anatomy. I believe that such an experience would convince any open-minded inquirer that like designations for homologous parts are desirable; and, secondly, he would be filled with increased admiration for the view of organic nature which points out the significance of a real likeness in what appeared in the beginning so utterly diverse. Here then is work which stands ready for the ablest zoologists.

In Embryology and Physiology the domestic animals have always furnished the greatest amount of information, as one can satisfy himself by consulting any treatise upon these subjects, although 'Human Embryology,' 'Human Physiology'

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may be printed on the title-page. Who did not get his start in embryology by studying the development of the chick, the dog, cat, rabbit, or guinea pig? And in physiology, students are almost equally dependent on the dog and rabbit. What is known in these fields is but a drop in the bucket, and as the domestic animals have contributed the greater part of that drop, so will they be called upon to fill the bucket to the brim. And what a splendid outlook there is at the present time. New discoveries in physics, like the X-rays, make possible advances in physiology. Perfection of technique in microscopy makes advance in embryology possible. Contemplate the opportunity and the promise for a moment. There is not a single treatise in any language which deals adequately with the embryology of the domestic animals, and the only one in English, the only one usually studied by the veterinary student, is hopelessly bad and antiquated. If one glances at the tables showing the zoological position of the different domestic animals he must be impressed with their wide distribution in the animal kingdom and their representative character. What an opportunity is here for work in comparative embryology? It is coming to be felt that the embryology of the present day is very inadequate in that, while it professedly deals with the entire development of the individual, it really devotes its main energy to the earliest stages and to the very beginnings of the organs. The complete ontogeny of the individual must go further than this and trace the development from the ovum through all the life stages to old age and death. It is only among the domesticated forms, in the higher groups at least, that abundant material under complete control, is at command, without very great expense. Abundant material, with full knowledge concerning it, will be required for satisfactory monographs in the future.

For students, material in great amount at a merely nominal cost, and without sacrificing animals especially for the purpose, may be had at every large abattoir; and every village slaughter-house wastes more than enough embryological material every year to supply the aspiring young zoologists in its vicinity. That this material is being utilized is evident from the admirable papers upon embryological subjects and from

the laboratory announcements of Harvard, Johns Hopkins, and many other centers for investigation and sound embryological instruction.

It was intimated above that the pressing need of zoology to-day is complete knowledge of some typical forms, such, for example, as are represented by the domestic animals in the avian and mammalian classes. This thorough knowledge is needed rather than more of the bits and patches from the entire animal kingdom. It is certainly true that morphological knowledge at the present day is too much like a crazy quilt. This, every investigator finds to his cost when he wishes to carry a research beyond the most elementary stages. What is needed, then, is concentration—complete knowledge, so far as possible, of each form investigated; and this knowledge must compass the entire life cycle. As also stated above, embryology has, and perhaps properly, concerned itself largely with the beginnings of the organisms and their organs. But in so doing the later but no less important changes have been left almost untouched. Ontogenetic development after birth is of the profoundest importance from all biological standpoints. In someways a knowledge of how the new-born becomes an adult is certainly of profounder interest than how an egg becomes a new-born animal. A few years ago the agricultural experiment stations, especially those of Wisconsin and New York, wished to answer, so far as possible, the question of how to obtain the best nutrition and growth to render animals most satisfactory as food and thus, also, the most profitable in the market.

There arose questions concerning the changes in muscle, if any, in passing from youth to maturity and from maturity to old age; from a condition of leanness to fatness. Here were some very pertinent questions which only a biologist could answer, but at that time many of the questions were enshrouded in darkness. But during the present year several investigations bearing upon these points have been published. Every one knows that the muscles increase in size as well as in strength in a growing animal, and also that they increase in size and strength in an adult if properly exercised. But who would have been prepared to expect that in this increase in

power and size of the whole muscle the individual fibers of which it is composed would actually decrease in number? This brings us to the fundamental question of the mechanism and the structural changes by which youth and maturity are merged into old age and decay. If you will read the suggestive address of Dr. Minot given at the Indianapolis meeting in 1890, and the papers of Hodge on the changes in nerve cells from childhood to senility, you will gain a notion of the work to be done upon the post-embryonal ontogeny, and the rewards to be gained by the faithful, clear-brained investigator.

I cannot leave this part of the subject without reminding you again of the brilliant part the paleontology of the horse has played in zoological science, and to express the belief that its embryology, when thoroughly worked out, will play an equally brilliant one. At present this embryology is known only in fragments. Why should there not arise in this boundless western world, in the land where the earliest horses appeared, some embryologist who, with the cheap and abundant material, should work out this problem with completeness? Next to man himself there is probably no animal in which the civilized world is more profoundly interested. To trace in the growing embryo not only its own life history, but to gather as many and clear glimpses as possible of its race history, would, indeed, be an inspiration. Enough is already known to make one sure that the field is worth working and that the harvest is certain. Almost as much might be said for some of the other domestic animals. And why should not some of this splendid work be done in America? This was the original home of the horse, of representatives of all the groups of domesticated animals, and every summer brings from its boundless treasures ever new and more marvelous forms. I believe that the time will come—indeed, that it is at hand—when zoological science, yes all science in America, will go forward with the giant strides which have already characterized her inventive and industrial history.

So far this address has been practically limited to the higher vertebrates, but I would not remain wholly silent upon the great phyla of invertebrates. The honey-bee and silkworm should not be passed by without a word. Their history, like

that of most of the domestic animals, is shrouded in darkness, but they are still with us, calling forth from each generation renewed interest and admiration. They, too, offer problems for the biologist, and deserve his attention. For example, take that great question of apparent voluntary parthenogenesis with the bees. What is the mechanism by which fertilized eggs become queens or workers and unfertilized eggs become only drones? Is this very general belief really true? If true what are the differences in the course of development in the eggs in the two cases? Then in Physiology what a multitude of problems the bees propound? Why will a special form of food cause an egg to develop into a queen instead of a worker? How can the workers change honey into beeswax? How can a mere blind pouch serve the purposes of digestion and excretion in the larva? For answering all these questions and many others the honey-bee is admirably adapted. One can keep the swarm constantly under his eye, and he can control, so far as necessary, the actions of the bees; there is abundance of material which may be had at all stages of development. Indeed, with the hundreds of thousands, perhaps millions, of insect species yet to discover and describe, and all these questions of structure, function, embryology, transformation, histolysis and redevelopment to answer, it looks as if the entomologist would not be compelled to sit down and sigh for new worlds to conquer for some time yet. And if I may be allowed to carry over my convictions from the vertebrates to the invertebrates, I believe that zoology would be far more advanced if a million or two species of insects were left undescribed and the enthusiasm and devotion of the entomologists—and no class of zoologists are more enthusiastic and devoted—were directed toward the elucidation of the entire life cycles of a few typical forms, and the structure, function and embryology of these were worked out as completely as modern knowledge and method would allow. Then there would be some standards of comparison to facilitate the work on the infinite number of forms still uninvestigated. From the monographs on the embryology and morphology of insects which have appeared during the last few years one cannot help feeling that this fascinating field will soon claim a multitude of students, and that none need go away empty-handed.

In Preventive Medicine and Hygiene the domestic animals have, as in so many other fields, served as the basis for study and investigation. To appreciate their importance one has but to recall the fact that at the close of the last century Jenner's application of cowpox as a protection against smallpox has led to an almost complete expulsion of this once-dreaded scourge from civilized lands; or to refer to the memorable investigations of Pasteur begun in 1866 for the amelioration of the condition of the silk industry of France. He saw and pointed out, with the greatest clearness, the importance of cleanliness, fresh air, and good food for the avoidance of degeneration and disease in the silkworms. Are not fresh air, cleanliness, and good food the very foundation stones of hygiene for all animal forms? In the silkworms, also, Pasteur found causes of disease in the microscopic organisms which infested their bodies. and in some cases at least this cause appeared to pass from one generation to the next through the eggs. What this study of Pasteur upon the diseases of silkworms, upon anthrax in the domestic mammals, upon fermentation, did for surgery is thus expressed by Lister, the recognized father of antiseptic surgery, at the jubilee celebration of Pasteur: "Truly there does not exist in the entire world any individual to whom the medical sciences owe more than they do to you. Your researches on fermentation have thrown a powerful beam which has lighted the baleful darkness of surgery, and has transformed the treatment of wounds from a matter of uncertain and too often disastrous empiricism into a scientific art of sure beneficence. Thanks to you, surgery has undergone a complete revolution, which has deprived it of its terrors and has extended, almost without limit, its efficacious power."

In our own and in other countries what untold loss has come from 'Texas Cattle Fever?' The working-out of the biological relations of that disease, it seems to me, is one of the most brilliant pieces of scientific investigation which has illuminated this truly luminous end of the nineteenth century. With all the knowledge accumulated since Pasteur's investigations on the silkworm diseases to serve as guides and to give suggestions, it took one of the foremost pathologists whom our country has produced (Dr. Theobald Smith) three years to bring

the investigation to a demonstration.   And little wonder ! For instead of the previously known simple relations of microbes to disease, the way was roundabout and involved two generations of animals and two species.   Furthermore, the germ of the disease was not a bacterium or fungus, easy to cultivate on artificial media, but one of the sporozoa for which no artificial culture medium has yet been devised.   The story is briefly as follows : Cattle ticks (*Boophilis bovis*) suck the blood of animals in which the Texas-fever germ is present.   The germs enter the eggs of the ticks and thus infect the next generation. This new generation of ticks attach themselves to other cattle and introduce into their blood the disease germs which are carried over from a previous generation.   And so the mutual infection goes on in a vicious circle from generation to generation.   The direct human interest, outside the economic one, which this investigation has is the suggestion and the accumulating proof that malaria in man is transmitted in practically the same manner by mosquitoes.   Truly the living hypodermic syringes are to be feared as well as execrated.

Thus hardly a triumph in medicine has been won without substantial aid from the domestic animals, and it is believed by the acutest minds engaged in the great work of ameliorating the sorrows of the world caused by preventable disease and premature death that we are now only on the threshold of discovery.   Is not the fact that the discoveries in medicine and hygiene in the past have been so dependent upon the domestic animals, sufficient guarantee that future discovery will be likewise dependent upon them ; and as human beings are so closely linked with the domestic animals in economics, in hygiene and in promised avoidance of disease, is there not abundant reason why the veterinary profession should be elevated and become a true unit in university life, a close colleague with the profession of human medicine ; and that human medicine in turn should reap even greater good in future by a more thorough appreciation and study of comparative medicine ?[1]

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[1] For further discussion of the relations between human and comparative medicine, see for Comparative Medicine, Dr. James Law's address at the inauguration of the New York State Veterinary College, September 24, 1896.—*Veterinary Magazine*, September, 1896.   For Human Medicine, see Dr. Charles S. Minot's Yale University Medical Commencement Address, June 29, 1899.—SCIENCE, July 7, 1899.

At this time, when the dawn of the twentieth century is already in the sky, the biological problem most important to the animals, and to the human race in its aspirations, is the problem of heredity. What is its mechanism, what light does it throw upon the chances for preservation from degradation, and for elevation to exalted manhood? Organic evolution has shown in the clearest manner that 'descent with modification,' in order to meet the requirements of the environment, does, not, by any means, signify in all cases what is commonly meant by the term progress. Consider the mental and physical condition of parasites. They have descended literally, and with the profoundest modifications. Look at the serpents and the partly limbless forms of the ocean. In their descent they progressed toward fitness for their environment, fitness to make the most and best of the life they have to lead; but this is not the modification desired in human descent. The Utopia for human society is where there is abundant food for all, congenial labor for all, education and amusement for all, every one to work out in its fullness his own individuality and at the same time serve the common weal. What lessons do the domestic animals give upon this? That 'like produces like' is a generalization believed in by every one, and sufficiently supported by every-day observation. Equally true and general is the statement that 'like produces unlike'—that is, no offspring is exactly like its parents, and no two offsprings are exact duplicates. While the race type is persistent, individual modifications are infinite. In this likeness and still unlikeness between offspring and parent is the hope and the despair of mankind. The hope because every horticulturist, every stock breeder, and every parent hopes that the offspring will be unlike, but that the unlikeness will be an improvement. The despair because unlikeness is just as liable to take the trend of the undesirable qualities and intensify them. With the lower animals the undesirable modifications may be eliminated, must be eliminated, or the race will deteriorate. In the human family the problem is equally plain, but infinitely more difficult of execution. How can the brood of criminals be avoided and the sturdy and right-minded possess the earth?

If one would see how social theories have worked themselves

out successfully the domestic animals again furnish models, models in which theory is no longer theory, but fact under which thousands of generations have lived, flourished, and passed away. The most perfect states are found among the social insects, foremost of which are to be mentioned the honey-bee. This society, which man has had under domestication so many thousand years that the beginning has been forgotten, has won the admiration of the world, and poets and philosophers have immortalized it with their words. What could appear more perfect? Each member of the society is apparently free, and each labors for the common good. Truly it seems an ideal state, but to attain this ideal state queens must kill their sisters or be killed by them ; thousands must be relegated to ceaseless toil, and kings exist but for a day. This perfect state consists only of a queen-mother and thousands of sexless slaves. All exist, not for their own individual pleasure, improvement, or happiness, but only for the community. If socialists will study this and other examples of states which have resolutely worked out the social problems to a successful finish they will perhaps get an inkling of how far off is the realization of all Utopias, of even the noble aspirations of our own National Declaration of Independence. Their realization is far off and difficult or impossible because the struggles of individualism are never compatible with perfect socialism. It is not possible to serve both the state and the individual with one's whole power. If there is partial service, as there must be in human society, neither the state nor the individual will have the most perfect development. The parallelogram of forces will give a resultant to be sure, but so far this resultant has proved a tortuous and unsatisfactory line instead of the perfect form of beauty dreamed of by the enthusiasts.[1]

In this brief review I have tried to show a few ways in which the study of domestic animals has thrown light on the problems confronting mankind in his social ideals, in preventive medicine, in physiology and hygiene, in embryology and comparative anatomy, and in the doctrine of the evolution of

[1] The reader who is interested in sociology is advised to read the admirable articles of Mrs. Anna Botsford Comstock on Insect Socialism in *The Chautauquan* for 1898, Nos. 4, 5, and 6; also Shaler's ' Domesticated Animals,' for their influence in civilization.

organic forms. The attempt has been made to show that, with the higher forms at least, that is the forms most closely related to man, and with whose destiny his own economic, hygienic, and social relations are most closely interwoven, the domestic animals have in the past and promise in the future to serve the best purpose because of the abundance of the material in quite widely separated groups of animals which long have been and still are under greatly differing conditions and surroundings; and, finally, because this material is plentiful and under control, and thus may be studied throughout the entire life cycle.

If any one is repelled from the study of domestic animals because they have been greatly modified by their so-called artificial surroundings in the company of man, I would remind him that man is also a part of nature, and that the modifications due to his action simply illustrate, in a somewhat definite and determinable degree, the plasticity of the forms under his control, and thus give the clearest and most undeniable proof of the capability of change in response to environment and selection. Furthermore, any wild form chosen for investigation has likewise departed widely from its primitive state, under the stress of changed and changing environment and a selection somewhat different but none the less severe. It is also contended that the knowledge of the environment of these domestic members of the zoological family for so long a time has been of the utmost help to many of the ablest workers, as one can infer from the quotation from Darwin in the earlier part of this address. There has been and still is too great a tendency in biology to study forms remote and inaccessible. This is, perhaps, partly due to the fascination of the unknown and the distant, and the natural depreciation of what is at hand. But study of these supposedly generalized types has proved more or less disappointing. No forms now living are truly primitive and generalized throughout. They may be in parts but in parts only. The stress of countless ages has compelled them to adjust themselves to their changing environment, to specialize in some directions so far that the clue through them to the truly primitive type is very much tangled or often wholly lost. Indeed, every group is in some features primitive. Even man himself is one of the best forms to study

the limbs upon. As expressed by one of my colleagues (J. H. Comstock) in his papers upon phylogeny, the unraveling of the mysteries of 'descent with modification' in their entirety cannot be worked out in a single form or group; the puzzle must be spelled out part by part, and one group will serve best for one organ and another for another.

As any complete study requires much material at all stages the higher forms must be of the domesticated groups, or wild forms must be practically domesticated for the time being to supply the material.

It may be objected, also, that in the investigation of domesticated forms sordid interests will play too prominent a part. No doubt, to the true scientific man the study of zoology for its own sake,—that is for an insight into the fundamental laws of life,—is a sufficient incentive and reward. Judging from the past, the study of the domestic animals in any other way than in a scientific spirit and by the scientific method will prove barren, but studied in that spirit and by that method the result has always justified the effort, and has thrown as much, if not more, light upon biological problems than an equally exact study of a wild form.

Therefore, while purely practical ends can never supply the inspiration to true scientific work, still surely no scientific man could feel anything but happiness that his work had in some ways added to the sum of human well-being. Perhaps no one has expressed so well the sympathy of a scientific man with his fellow men as Pasteur in the preface to his work on the silkworm diseases: '' Although I devoted nearly five consecutive years to the laborious experimental researches which have affected my health, I am glad that I undertook them. * * The results which I have obtained are perhaps less brilliant than those which I might have anticipated from researches pursued in the field of pure science, but I have the satisfaction of having served my country in endeavoring, to the best of my ability, to discover a remedy for great misery. It is to the honor of a scientific man that he values discoveries which at their birth can only obtain the esteem of his equals, far above those which at once conquer the favor of the crowd by the immediate utility of their application ; but in the presence of mis-

fortune it is equally an honor to sacrifice everything in the endeavor to relieve it. Perhaps, also, I may have given young investigators the salutary example of lengthy labors bestowed upon a difficult and ungrateful subject."

As a final word, let me summarize this address by saying: However necessary and desirable it may have been in the past that the main energy of zoologists should be employed in the description of new species and in the making of fragmentary observations upon the habits, structure, and embryology of a multitude of forms, I firmly believe that necessity or even desirability has long since passed away, and that for the advancement of zoological science the work of surpassing importance confronting us is the thorough investigation of a few forms from the ovum to youth, maturity, and old age. And I also firmly believe that, whenever available, the greatest good to science, and thus to mankind, will result from a selection of domesticated forms for these thorough investigations.[1]

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[1] If the young zoologist wishes to get a clear notion of the meaning and value of 'species' in modern biology he is recommended to read Dr. Farlow's address in last year's Proceedings; also Dr. D. S. Jordan's 'Kinship of Life' in his 'Foot-notes on Evolution,' and Professor Bailey's chapter on 'Experimental Evolution Amongst Plants' in his book on the 'Survival of the Unlike.'

PAPERS READ.

[ABSTRACTS.]

The Utility of Phosphorescence of Deep Sea Animals. By C. C. Nutting, Univ. of Iowa, Iowa City, Ia.

[Published in the *American Naturalist* for October, 1899, pp. 793-799.]

The paper is an attempt to explain phosphorescence in terms of utility to its possessors.

When possessed by free swimming forms it acts in a manner analogous to "alluring coloration " in some cases. In others it reveals the prey, in others it may be " directive," and in still others protective in function. Among the Protozoa it may serve to keep individuals of a species together, and thus secure conjugation.

When possessed by fixed forms, such as many cœlenterates, the phosphorescence does not serve the purpose of warning coloration, nor is it useful to attract the mate, or sex elements of opposite sexes, but it is useful in attracting many organisms that serve as food for its possessors.

The Course of the Fibres in the Optic Chiasma of the Common American Toad, Bufo lentiginosus. By B. D. Myers, Asst., Dept. Comp. Phys., Cornell Univ., Ithaca, N. Y.

1. The decussation is total.

2. The chiasma is made up of a crossing of fibers and not of bundles of fibers as described by the earlier writers.

3. There is not that gradually increasing complexity of decussation in the chiasma from fishes to mammals as described by Wiedersheim.

4. There are no interretinal fibers.

5. The trophic center for at least the greater part of the fibers of the optic nerve is in the retina.

6. On experimental grounds the toad is capable of monocular vision.

7. After loss of sight of one eye, contrary to the old belief, the toad does not die of starvation.

8. Flemming's fluid hardening is superior to Marchi's method in that the normal nerve tracts are absolutely free from those blackened granules so closely resembling degeneration.

9. Degeneration after severance of the optic nerve of the toad is first seen after five days.

On Reighardia, a New Genus of Linguatulida. By Prof. Henry B. Ward, University of Nebraska, Lincoln, Nebr.

In the air sacs of gulls on Lake St. Clair was discovered in 1894 a vermiform parasite which could not be definitely placed. It occurred infrequently and even when found was present in small numbers. The host was *Bonaparte's Gull*. Last year the same parasite was found in the common tern on Lake Erie. Here it was even rarer, only one bird in 100 being infested. One of three parasites obtained was a female containing well-developed embryos and from their character it was easy to determine the taxonomic position of the parasite as closely related to the Linguatula. Subsequent careful study showed also the characteristic hooks of the family, yet very poorly developed. The body is elongated, cylindrical, transparent, and devoid of any annulations. The cuticula is thin, bearing around the mouth-opening a chitinous framework recalling that of the Sarcoptidæ. Of its post-embryonic development nothing is known.

Our species of Linguatula is recorded from a gull of Arctic Ocean. This form which is incompletely described probably belongs to this new genus, a view strengthened by some minor details mentioned by the author.

Photographing Natural History Specimens under Water or Other Liquids with a Vertical Camera. By Prof. S. H. Gage, Cornell University, Ithaca, New York.

The purpose of this paper was to show by specimen photographs what could be done in getting accurate pictures of live aquatic animals and of delicate specimens which must be supported in liquids. By means of a vertical camera, this is as easily accomplished as photographing in the ordinary horizontal position. The most notable photograph, perhaps, was that of a live fish with five just transformed lake lampreys attached.

On Some Heteropterous Insects Formerly Responsible for Spider Bite Stories. By Dr. L. O. Howard, Entomologist, U. S. Dept. Agriculture, Washington, D. C.

[Published in *Appleton's Popular Science Monthly*, November, 1899, pp. 31-42.]

CAVE ANIMALS: THEIR CHARACTER, ORIGIN, AND THEIR EVIDENCE FOR OR AGAINST THE TRANSMISSION OF ACQUIRED CHARACTERS. BY CARL H. EIGENMANN, PH.D., Prof. Zool. and Director of the Indiana University Biological Station, Bloomington, Indiana.

Cave faunas bear the same relation to those of the neighboring regions that island faunas bear to those of neighboring continents, but caves are not as a rule colonized by accident. As far as the vertebrate fauna is concerned only those types are represented which in their epigean existence were negatively heliotropic, and caught their food under rocks, under logs and in crevices, the amblyopsid salamanders, rats, etc. A gradually disappearing light leads to an increase in the size of the eyes; *viz.*, nocturnal animals in general; whereas the gradual diminution of the use of the eyes leads to the reduction of their size and the simplification of their histological complexity whether light is present or not.

The degeneration, if it takes place in the light, affects first the retina, then the dioptric arrangement, and finally the purely passive structures, as the scleral cartilages. The lens, after it is once affected degenerates much more rapidly than the other elements, and usually disappears before the other structures vanish. The evidence from the differential degeneration is for the transmission of disuse effects. The habits and structure of the species of blind fishes and the differential degeneration mentioned eliminate the possible influence of natural selection, panmixia, compensation of growth principle, or germinal selection, as factors in the phylogenic degeneration of these eyes.

HAVE WE MORE THAN A SINGLE SPECIES OF BLISSUS IN NORTH AMERICA? BY PROF. F. M. WEBSTER, Ohio Expt. Station, Wooster, O. [Published in the *American Naturalist*, October, 1899, pp. 813-817.

ÆSTIVATION OF EPIPHRAGMOPHORA TRASKII IN SOUTHERN CALIFORNIA. BY MRS. M. BURTON WILLIAMSON, Los Angeles, California.

An experiment with two *Epiphragmophora Traskii* to ascertain how long these snails suspended in the air would remain with the functions of digestion and respiration in a state of torpor, and, also to compare their longevity with other helices of the same colony, kept in a snailery in the garden.

NATURAL TAXONOMY OF THE CLASS AVES. BY R. W. SHUFELDT, M.D., Takoma, D. C.

This paper briefly takes into consideration the origin of birds as a

group, as evidenced by the structural organization of its living members, and a study of such fossil material as has fallen into the hands of science. An historical sketch is presented giving the main features of the various schemes of classification of the class in times past by the best recognized authorities, with critical comments thereon. The peculiar difficulties attending the classification of birds is contrasted with the problem as presented by other groups of the Vertebrata. Such morphological characters as best subserve the purposes of avian taxonomy are examined into and compared, with brief notes upon their significance and value. A scheme of classification of the class Aves is presented wherein osteology has been the main anatomical system used although by no means to the exclusion of the remainder of the bird's structure.

NOTES ON THE CHICK'S BRAIN. BY MRS. SUSANNA PHELPS GAGE, Ithaca, N. Y.

A systematic review of the development of the brain of the chick has been undertaken with the end in view of determining the real value of the furrows upon the mesal or endymal surface of the brain tube. In the earliest stages while the dorsal union is taking place there is a furrow demarkating the region adjacent to the neural crest from the remainder of the nervous tube and ending in the optic cup. As the neural crest separates into ganglia which grow down the side of the tube this furrow disappears and the brain tube assumes the well-known moniliform with total folds of the wall.

Of the five transverse furrows of the oblongata, two certainly leave a remnant which can be traced definitely to the 9th and 10th days. To that time there is not reestablished a continuous longitudinal furrow throughout the brain tube separating the so-called dorsal and ventral zones of His, but in each portion of the brain arise furrows of limited extent which, from comparison with other brains, promise to prove of value in homologizing parts.

FURTHER NOTES ON THE BROOK LAMPREY (LAMPETRA WILDERI). BY PROF. S. H. GAGE, Cornell University, Ithaca, New York.

In this paper were brought out the additional facts bearing upon the non-parasitic habits of the adult brook lamprey. (1) It was shown that the lake lamprey, as soon as completely transformed, attacked fish with great ferocity. (2) Transformed brook lampreys under the same conditions never attacked the fish. (3) The alimentary canal of the lake lamprey was comparatively large with many secondary, longitudinal folds at

the time of transformation, while that of the brook lamprey was very small and quite or almost completely unfolded.

Bearing upon the question of possible ancestral parasitism in the brook lamprey, serial sections were made of the larvæ at the beginning of transformation, when the transformation was nearly complete, and of the adult at the spawning season. It was found : (1) That the branchial apparatus undergoes the same modification as in the lake lamprey in that the common branchial chamber becomes divided into seven branchial pouches on each side, and the formation of a common median branchial canal opening into the mouth and by passages into each branchial pouch. (2) That there is developed an esophagus connecting the mouth with the alimentary canal as in the known parasitic forms (lake and sea lamprey). (3) The buccal and lingual armature of horny teeth are well developed.

It was concluded that the indications all point to an almost certain conclusion that the progenitors of the brook lamprey were true parasites although at present the brook lamprey possesses an esophagus which is not used for swallowing food and buccal and lingual teeth which are no longer used for lacerating prey, but remain as stigmata of an ancestral mode of life.

RESPIRATION IN TADPOLES OF THE TOAD (BUFO LENTIGINOSUS). BY PROF. S. H. GAGE, Cornell University, Ithaca, New York.

On comparing the behavior of toad tadpoles with that of the tadpole of the bullfrog, for example, one is surprised to find that the toad tadpoles go to the surface with far less frequency, and if the water is very fresh they may appear not to go to the surface at all. In a small glass vial they may remain at the bottom for half an hour or more. An investigation of the development of the lungs showed that they appear very early ; that is, long before the hind legs, but it was found that the opening of the trachea into the mouth through the glottis, and the development of the larynx did not occur until the tail was nearly absorbed. From this structural condition there could be no aerial respiration by the lungs in the tadpole state as the lungs do not communicate with the exterior, but are closed sacs.

The apparent aerial respiration of the toad tadpole is explicable only on the ground that air is taken in and mixed with the water which passes over the internal gills, something as fish go to the surface and gulp air when the air dissolved in the water is too nearly exhausted.

A DISCUSSION OF ASPIDIOTUS CYDONIÆ AND ITS ALLIES. BY C. L. MARLATT, First Assistant Entomologist, U. S. Dept. of Agriculture, Washington, D. C.

[Published in full in the *Canadian Entomologist*, 1899. pp. 208–211.]

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EFFECTS OF HYDROCYANIC ACID GAS UPON ANIMAL LIFE, AND ITS ECONOMIC USE. BY PROF. W. G. JOHNSON, State Entomologist, College Park, Md.

A preliminary report upon a series of experiments with this gas upon animal life.

THE HISTOGENESIS OF MUSCLE IN THE METAMORPHOSIS OF THE TOAD (BUFO LENTIGINOSUS AMERICANUS). BY B. F. KINGSBURY, Ass't Professor of Histology, Cornell Univ., Ithaca, N. Y.

The author spoke of the occurrence of metamorphosis in the development of certain animals, among them the toad ; the necessity of changes in metamorphosis,—histolysis and histogenesis of the tissues, etc.; the views on the changes constituting histogenesis of tissues, muscle especially ; the results of work on the toad and frog ; on the bearing of these results on general biological principles.

THE PROGENITORS OF BATRACHIANS. BY DR. THEO. GILL, Washington, D. C.

Gave evidence that the Batrachians are probably descended from a type of fishes most nearly represented in the present fauna by the Polypterids.

OBSERVATIONS ON THE VARIATIONS, LIFE HISTORY, AND HABITS OF A MIMETIC LOCUST (ŒDIPODA MARITIMA). BY PROF. HERBERT OSBORN, Ohio State University, Columbus, Ohio.

Discussion of the variations and possible factors in affecting variations in a locust which shows striking protective resemblance, and some observations regarding its habits and life history.

A CHART FOR ILLUSTRATING THE ORIGIN AND EVOLUTION OF ANIMAL AND VEGETABLE LIFE. BY DR. A. D. HOPKINS, Vice Director and Entomologist, W. Va. Agric. Expt. Station, Morgantown, W. Va.

An original scheme for illustrating theorie on the origin and evolutions of forms, genera, families, orders, etc., of life, is illustrated by means of a disk divided into spaces of various sizes and forms by curved and straight lines rising from the center of the disk.

GEOGRAPHICAL VARIATION, AS ILLUSTRATED BY THE HORNED LARKS OF NORTH AMERICA. BY HARRY C. OBERHOLSER, Biological Survey, Dept. Agriculture, Washington, D. C.

The distribution of the horned larks ; their relation to faunal areas ; their distribution compared with other plastic groups ; geographical variation in the horned larks, and comparison of variation in other groups ; anomalies in variation of the horned larks ; an examination into the causes of geographical variation.

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ADDRESS

BY

CHARLES R. BARNES,

VICE-PRESIDENT AND CHAIRMAN OF SECTION G.

THE PROGRESS AND PROBLEMS OF PLANT PHYSIOLOGY.

There are some subjects of whose content and extent most educated people have fairly accurate conceptions, though they may not appreciate the significance of the numerous problems which those who carry forward research are attacking. Literature and history number their appreciative amateurs by thousands. Even such sciences as astronomy and chemistry receive a fair measure of popular approbation and are widely appreciated.

Unhappily this is not yet the case with botany. By even the limited number who think they know of what it treats, it is frequently misunderstood and consequently undervalued. To most mature people it is hardly more than a name for a dilettantish dissecting of flowers, for which an apprenticeship of memorizing troublesome technical terms must needs be served.

It is easy to discover why this is so. It has resulted from the mistaken ways of presenting the subject to elementary pupils. But the difficulty of correcting the misapprehension is not decreased by a knowledge of the way in which it has arisen. We can only rely upon the gradual substitution of better ideas in the newer generation by means of more adequate instruction, and on the occasional popular presentation

of more accurate information. For the former we may look to the schools, which are rapidly changing the scope of their teachings. The latter, however, should be undertaken by specialists, as a matter both of duty and privilege. Popular accounts of plant phenomena may be accurate without being dull, interesting without being sensational, and attractive without being sentimental. We can only expect these qualities, however, in properly qualified writers whose scientific training has been sufficient to kindle their enthusiasms and quicken their energies,—without spoiling their English. Such books, in considerable number, have been appearing lately from American writers. May their tribe increase! As books of this kind are multiplied we may hope for an increasing appreciation of the science of botany both educationally and economically.

When the general subject has been so misapprehended, what can be expected regarding one division of it? The experimental study of the physiology of plants is not new. Hales more than a century ago carried out such accurate experiments that they are quoted to-day. But even to fairly educated people the word physiology, conjoined with plant, conveys no definite idea. "Physiology" we studied at school; is it not that hybrid of human anatomy and hygiene, with barely enough real physiology to salt it, which is inflicted upon immature youngsters, to the accompaniment of lurid lithographs of an inebriate's stomach? But what can "physiology" have to do with plants, that have no teeth to decay, stomachs to ulcerate, or eyes to become myopic? And so it comes about that one must explain to the average man that plants are really alive, that they work and rest, that they are sensitive to what goes on around them, and that they have established relations with their plant and animal neighbors. How they do these things and how their activities underlie those of all other living beings, even man's, lies within the compass of this branch of the science of botany.

But to such a company as this it is not necessary to set forth in detail the province of plant physiology or to justify its rapid introduction into institutions of higher learning. While Caesalpino and the school men argued vainly about the location of the "soul" of plants, it was the growing dissatisfaction with the

empty reasonings of such scholastic philosophy that drove men to observe the phenomena of nature. Thus real physiology had its birth in the last half of the seventeenth century, only a little later than the other natural sciences. It is, however, only in comparatively recent years that plant physiology came to be established upon a firm experimental basis and became fitted to take its proper place among the sciences offered in university curricula. Its real and vigorous growth has been measured by scarcely four decades. Among the countless results of the rejuvenation of biology wrought by various co-operating causes about the year 1860, may be enumerated the rise of plant physiology. One of the first evidences of this renascence was the publication in 1865 of Sachs's "Handbuch der Experimental-Physiologie," the first volume which gave any comprehensive and clear view of the phenomena of plant life.

From that day to this, with increasing vigor, Sachs's countrymen have been prosecuting researches into plant doings and guiding many students in their maiden investigations. French, Austrian, Italian, and Russian students have also made notable advances. On the continent a few great centers of physiological research have been developed like Würzburg, Tübingen, Leipzig, Bonn, Berlin, Vienna, Prag, and Paris. Great Britain has made a notable beginning at three of her great university centers.

But in this country the specialization which alone makes possible the effective development of a subject, has been slower in coming, and it is scarcely a decade since physiology began to have any considerable attention. Five years ago (I speak by the card) one could count on the fingers of one hand the colleges which offered any but brief lecture courses in plant physiology, and the number giving even lecture courses was less than 4 per cent. of the total number of colleges! I am sure that many in this audience would be surprised were I to recite the long list of prominent institutions which gave no physiological courses—*some even no botany!* In late years many have made a beginning in the way of demonstration and lecture courses, but the number with even fairly equipped physiological laboratories is still few. Indeed, there are to-day not twenty-five institutions of higher learning in the United States which offer

laboratory instruction in plant physiology, even in an elementary way, and still fewer which give opportunity for as much as a year's work. Graduate work in physiology, if the Graduate Handbook for 1898–9 may be relied upon, is now offered only at Barnard, Chicago, Columbia, Harvard, Michigan, Minnesota, and Pennsylvania. The development of centers of physiological research is therefore a matter of the future. It cannot be long delayed, however, for there is noteworthy energy in the advancement of this subject in several of the stronger institutions.

To the professional botanists, who are especially concerned in the advancement of the science, it would doubtless be of some interest should I take this opportunity to recapitulate the investigations which have been most fruitful of progress in the past decade. But the field is so vast, and work is being so vigorously prosecuted that I should despair of being able, within the limits custom sets, to present adequately the march of our knowledge of plants within the last decade. To such a task, moreover, my own knowledge would be wholly inadequate.

Therefore, instead of presenting a summary of so extensive investigation, I choose rather to confine my attention to the physiological aspects of botany, and in this field to endeavor to bring before you a conception of the general *trend* of investigation, without any endeavor to mention the work of individuals or even the important isolated researches which may be the starting points of new lines of progress. At the same time I shall seek to indicate what I conceive to be fruitful lines of study and shall direct attention to some of the unsolved problems which still confront the physiologist.

PHYSICAL CHEMISTRY.

The physiologist is dealing with material phenomena as manifested by living things. Physiology is, therefore, chiefly the application of the knowledge of chemistry and physics to the phenomena of life. It follows that the physiologist must be familiar with the laws deduced by chemists and physicists from their study of matter which is not under the influence of life. He needs to be equipped with the best physical and chemical knowledge of the day. Because of a want of such

training reproach has often fallen upon physiology in the past. Inattention to these underlying sciences has led to divers fantastic explanations of phenomena—explanations forbidden by the fundamental facts of chemistry and physics. Compelled thus to rely on advance in other sciences for the possibility of progress in their own, physiologists welcome with the brightest anticipations the rapid growth and development of that field in which chemistry and physics merge — physical chemistry. There is much, it is true, with which its students concern themselves that does not touch directly the activities of plants. But some of its subjects are of the most intimate concern to physiologists.

Solutions.—This is notably the case with the comparatively recent coordination of long-known facts and late discoveries into clear and definite laws of solutions. In no condition, outside and inside the plant body, does matter play a more important physiological rôle than in a state of solution in water. The prevalence of a cellulose wall, jacketing the protoplasm of their cells, is probably the most characteristic mark of plants. This membrane precludes the entrance into the body of any substance not in solution, whether originally solid or gaseous. Thus the behavior of solutions is of fundamental importance for the absorption of foods by the colorless plants, and of the raw materials out of which the green plants can make foods.

The cellulose wall has been adapted by plants to subserve a function, unknown in the animal body, namely, turgor. Only a knowledge of solutions enables us in a measure to understand the existence and regulation of turgor. The solutions enclosed by the semipermeable protoplasmic membrane of the living cell are rarely or never the same as those outside the plant or in paths of water conduction. Such a condition may establish at once a movement of water into the cell and develop a definite amount of hydrostatic pressure, equivalent to the osmotic pressure of the dissolved substances. Thus, by a figure, it is said that the osmotic pressure of the internal solutions pushes outward the protoplasm, backed by resistant but elastic wall, which stretches until its elastic resistance balances the osmotic pressure. If the cell be one of a group the cohesion and turgidity

of the cells surrounding any one resists its enlargement. Thus all the cells of a turgid mass of tissue bear firmly against one another, and this condition is of great importance in maintaining the form of young parts in which as yet no mechanical tissues exist. Turgor has its influence also in regulating the diffusion of water vapor through the stomata, in transfusing liquid water through water-glands, in certain forms of secretion and so on. So important is turgor that special salts seem to be provided to maintain it at a normal point. Its relations to growth also are unquestionably of prime importance, but we are not able at present to interpret these relations satisfactorily. Although the statement is generally made that turgidity is a prerequisite for growth and regulates it, there are some strong reasons for thinking that the relation is rather the reverse, and that growth regulates turgor.

Pathological changes may also be brought about by abnormally high osmotic pressure, a notable instance being furnished by œdema of various organs, especially leaves. In such a case, turgor seems to distend the cell walls extraordinarily, and to act as a stimulus on growth, causing a local hypertrophy characterized by bladdery tissues.

For interpreting all these processes, most fundamental for nutrition and growth, the new knowledge of solutions furnishes invaluable aid. This theory, developed mainly within the last decade by the labors of Pfeffer, van 't Hoff, Arrhenius, Ostwald, Raoult, and others, looks upon a substance in solution in water as essentially a gas. Its molecules are freer to move than they are in the solid state because of their relations to the molecules of water. These, at the same time that they make mobility possible, obstruct the movements of the solute, so that the molecules of the latter are not nearly so free to move as the molecules of a gas. Thus enormous pressures are necessary to move the solute through the solvent or to remove its molecules from it. Many demonstrations establish firmly the fact that the molecules of solutes exhibit the well-known laws of gases. This general applicability of the fundamental laws of gases to solutes has made evident the proper basis of comparison between solutions of different compounds. For many years, and for some years after a proper knowledge of physi-

cal chemistry would have led to their abandonment as not comparable, physiologists were comparing the physiological action of percentage solutions or solutions of definite specific gravity, in ignorance that this was like comparing the action of one gas at atmospheric pressure with that of another at 10 atmospheres' pressure. Henceforth, we must deal with equimolecular solutions if a comparative knowledge of physiological action is sought.

A further study of the behavior of solutions has made us acquainted with the fact that when water solutions which conduct electricity, i. e., electrolytes, are of less than a certain concentration, the solute undergoes partial dissociation, no longer existing alone as a definite chemical compound. A certain amount, depending on the concentration of the solution, is broken up into electrically charged part molecules or ions, which behave osmotically as molecules and increase the osmotic pressure of the solute. Moreover these ions exert a very marked physiological effect upon the protoplasm. Certain ions are extremely injurious, inhibiting the activity of the protoplasm and resulting in death. Poisons, so-called, produce a similar result. It is possible that by a study of ionic action we may obtain a more accurate idea of what actually happens when living matter dies by "poison." It would be surprising were there not a considerable diversity in the actual effects of various "poisonous" agents.

Again, certain ions have a less marked physiological action, which amounts only to stimulation, calling forth corresponding change in the activity of the protoplasm. Unquestionably many of the peculiarities of growth and development of an organism are responses to the action of ionic stimuli, but of these practically nothing is yet known. Certain human sensations have already been shown by Kahlenberg in his investigations on taste to be due to the action of definite H and OH ions. In no organisms is there so good an opportunity as among plants to determine precisely how these factors, always acting in complex combinations, effect the modifications of form and function that constitute adaptation to external conditions.

Studies of this kind have barely begun. Kahlenberg and

True were the first to establish the poisonous action of ionic hydrogen in solutions of certain acids and salts. A few other observers have attacked similar problems, but the field is hardly yet explored; it has not been at all cultivated. The relations are complex, it is true, and their unraveling will not be easy; but surely there are rich harvests for the patient worker.

In the light of the modern theory of solutions it is essential that the whole field of root absorption be reexamined. Dilute solutions of the soil must surely be electrolytically dissociated in large measure, and this fact doubtless stands in intimate relation to the entrance of solutes into the plant. In the absence at present of complete experimental demonstration of the behavior of these substances, we are compelled to rely largely upon theoretical probabilities. Interesting possibilities, however, present themselves to the speculative worker and point out various directions in which investigation may be fruitful.

Energy.—One of the directions in which physical knowledge is now extending, but in which it is still so imperfect as to leave much to be desired, is in the understanding of the forms and transformations of energy. But the physiology of plants has not yet made use of all the knowledge that is available in this direction. Though in the past decade we have had some important researches, there yet remain great gaps in our knowledge of the income of energy to the plant and of the ways in which it is utilized. I may here indicate only a few of these gaps in our knowledge.

While it is easy to calculate the potential energy of the foods absorbed it is not easy to determine how much of the energy is available, in what form it is released, and what changes it undergoes as it is used by the plant.

We know that heat is one form of energy which is constantly affecting the organism, and we speak of certain temperature limits as one of the essential conditions for life. But what does that mean? Why is it a condition of life? Is it merely because the necessary chemical changes can only occur within certain limits? If so, what does *this* mean? Does it mean that the radiant energy which imparts to us the sensation of heat must be acting upon the molecules of the various chemical compounds ere they are capable of enough lability to afford the

living protoplasm opportunity to push them over, so that they fall into simpler compounds, or to lift them to a higher level of complexity and to greater instability? If heat does not merely increase chemical instability, is life possible within certain limits of temperature because there is pouring into the organism a supply of energy which the protoplasm may utilize in directer fashion to do the work necessary to existence?

What is the source of energy for the colorless plants which assimilate the simpler foods? It is almost inconceivable that they can produce proteids out of the carbohydrate and nitrogen compounds with which they can be supplied without needing a considerable amount of energy besides the potential energy which reaches them in the foods they absorb. If there is no direct supply of radiant energy, it looks very much as though these plants had acquired the long-sought power of lifting themselves by their own boot straps. Yet if radiant energy, either as light or heat, is utilized by them, we know nothing of it at present. Or is it the energy of the O, absorbed for respiration which accounts for the extra work done? The data are not at hand to determine the correct answer to these questions. General statements abound and to many it may seem that all this is known, since it is often dogmatically settled in text-books. Yet in reality we must have exact measurements of the amounts of energy involved—a thing not yet accomplished—before we can be said really to know whence plants derive their energy and what heat means for them.

Even the case of the green plants is not at all clear. That they construct their own food in great measure is certainly true. That they do this by using absorbed radiant energy of the quality which gives us the sensation *light* is well known. But it is by no means clear in terms of chemistry and physics how this is done, nor even what measure of the absorbed energy is utilized. Measurement, indeed, is difficult, yet quantitative results are necessary before we can be satisfied that we know what is happening when the leaf makes food.

Finally it may be said that little is yet known of the energy relations in the processes of growth. Here, since we must deal wholly with internal release and utilization of energy, investigation will be most difficult and uncertain.

Stereochemistry.—The decade that is passing has witnessed the very great extension of chemical knowledge in the direction of the constitution of the molecules of carbon compounds. Stereochemistry touches plant life most obviously in its relation to the carbohydrates which are constructed by the green plants, and digested and utilized by all. The phenomenal work of Fischer on the sugars supplemented as it has been by that of Tollens, Kiliani, Lobry de Bruyn, and others on asymmetric carbon atoms, has put us in possession of facts which throw a flood of light upon nutrition and are destined, when more completely exploited and fully applied, to elucidate many difficulties in our present thinking about the feeding of plants.

We have learned, for example, that a carbon compound, to be a valuable food, must not only contain C, H, and O, but that these must be combined in a particular fashion. The aldehyde group CH:O, the ketone group CO, and the radical CH$_2$OH are characteristic of good foods. The simpler sugars such as glycerose and arabinose ; the hexoses, glucose or grape sugar, fructose or fruit sugar, mannose and galactose; the polysaccharides, sucrose or cane sugar, lactose or milk sugar, and maltose or malt sugar are all substances which have been proved useful as plant foods, and all contain one or more of these groups.

Up to a certain limit, the presence of a particular molecular group increases the food value. What does this phrase "food value" mean? Does food value depend solely on availability of energy, *i. e.*, the ease with which it can be released? Or has the form in which energy is set free something to do with its availability and the consequent food value? Or does the constitution of the molecules before and after decomposition affect food value? If so is it because the constitution of the molecules is related to the form in which energy is released or because it is related to the ease with which energy is released?

When the complex carbohydrates like starch and inulin are to be utilized, they break down through a series of dextrins and levulins respectively, finally becoming simplified to hexose sugars. Why is this necessary? And how are we to interpret these decompositions? Are they part of the energy-release? It can hardly be doubted that the constitution of the

molecules of starch and inulin, composed respectively of units of glucose and fructose, determines the permanence while in the storage form, and that separation into their constituent units in digestion makes possible the assimilation of the sugars as food. It is plain, therefore, that a precise knowledge of the constitution of starch and inulin is a desideratum. We must look forward also to further extension of stereochemical knowledge of the almost infinite variety of the other carbon compounds and to the investigation of the nitrogenous substances, as yet scarcely well begun. These may be expected to put physiologists into possession of valuable clues to the secrets of nutrition and respiration.

How intimate this relation between the arrangement of atoms in space and physiological activity is, is to be seen in the fact that fermentability is dependent upon the configuration of the sugar molecule. It has been found that, of the many sugars known, only those with 3, 6, or 9 atoms of carbon in the molecule are fermentable. Thus the triose sugar glycerose, whose formula is $C_3H_6O_3$, is fermentable, while the tetrose sugar erythrose, $C_4H_8O_4$, and the pentoses, ribose, lyxose, xylose, and arabinose, $C_5H_{10}O_5$, are not. In like manner several of the hexoses, $C_6H_{12}O_6$, and the nonnoses, $C_9H_{18}O_9$, are fermentable, while the intermediate ones, such as the heptoses, $C_7H_{14}O_7$, are not.

But the relation is still more intimate. Even when the proper number of atoms is present they may be arranged in such a fashion as not to be open to disturbance by an organism.

Thus, certain species of yeast are capable of fermenting *d*-glucose, *d*-mannose, and *d*-galactose. The arrangement of their molecules may be represented in a plane as follows:

$$\begin{array}{ccccc} & H & H & OH & H \\ CH_2OH - & C - & C - & C - & C - COH \\ & OH & OH & H & OH \end{array}$$
d-glucose.

$$\begin{array}{ccccc} & H & H & OH & OH \\ CH_2OH - & C - & C - & C - & C - COH \\ & OH & OH & H & H \end{array}$$
d-mannose.

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$$CH_2OH - \overset{\displaystyle H}{\underset{\displaystyle OH}{C}} - \overset{\displaystyle OH}{\underset{\displaystyle H}{C}} - \overset{\displaystyle OH}{\underset{\displaystyle H}{C}} - \overset{\displaystyle H}{\underset{\displaystyle OH}{C}} - COH$$
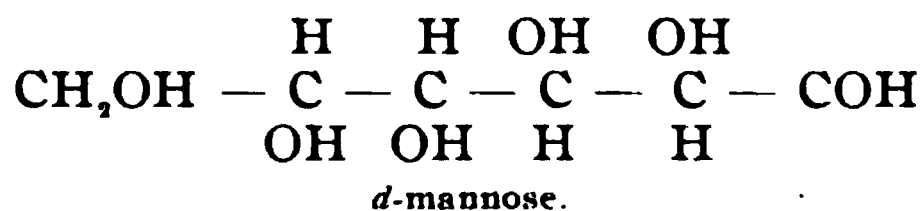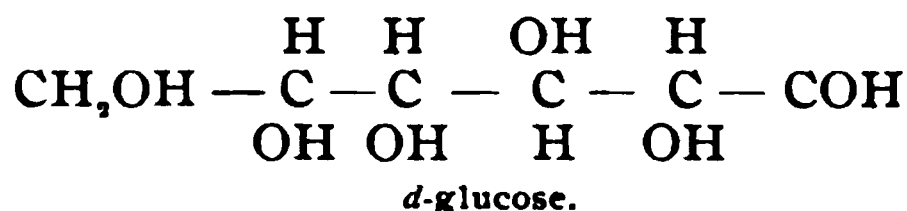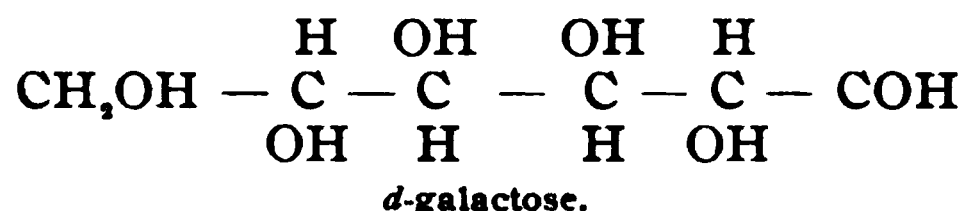
d-galactose.

But *d*-talose, whose structure is the following,

$$CH_2OH - \overset{\displaystyle H}{\underset{\displaystyle OH}{C}} - \overset{\displaystyle OH}{\underset{\displaystyle H}{C}} - \overset{\displaystyle OH}{\underset{\displaystyle H}{C}} - \overset{\displaystyle OH}{\underset{\displaystyle H}{C}} - COH,\ \text{cannot be fermented by}$$
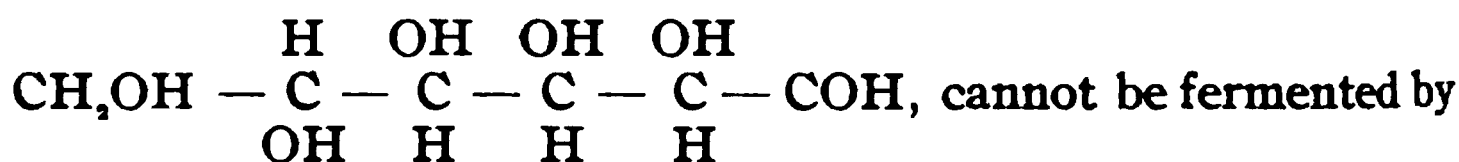
these yeasts. Inspection shows that *d*-talose differs from *d*-galactose and *d*-mannose only in the transposition of the molecular groups about a single one of the asymmetric carbon atoms, and from *d*-glucose only in the transposition about two carbon atoms.

Again while *d*-glucose, $CH_2OH - \overset{\displaystyle H}{\underset{\displaystyle OH}{C}} - \overset{\displaystyle H}{\underset{\displaystyle OH}{C}} - \overset{\displaystyle OH}{\underset{\displaystyle H}{C}} - \overset{\displaystyle H}{\underset{\displaystyle OH}{C}} - COH,$
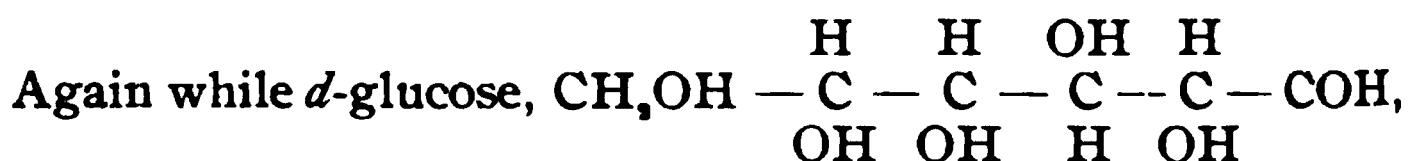
is fermentable, its isomer, *l*-glucose,

$$CH_2OH - \overset{\displaystyle OH}{\underset{\displaystyle H}{C}} - \overset{\displaystyle OH}{\underset{\displaystyle H}{C}} - \overset{\displaystyle H}{\underset{\displaystyle OH}{C}} - \overset{\displaystyle OH}{\underset{\displaystyle H}{C}} - COH,\ \text{is not at all fermenta-}$$
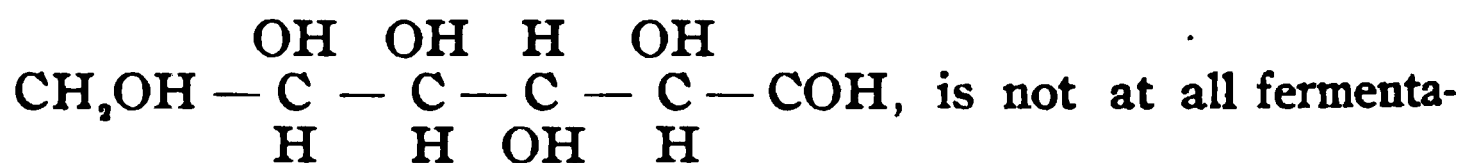
ble. The same is true of *l*-fructose and *l*-galactose.

The discovery that yeasts, so long believed to show direct ferment action of the protoplasm, produce the chemical changes known as fermentation by the intervention of enzymes, removes the problem from the immediate field of physiology, only to group it with the host of baffling catalytic phenomena which the chemist is at present wholly unable to explain. Thus all fermentations at present known become closely associated with the digestive processes in nutrition. We may scarcely expect light upon all these phenomena until the preparation of the enzymes in a state of purity is attained. This, it is to be hoped, will be followed by a knowledge of their composition, though, as they now appear to belong to the group of nucleo-proteids, this may only be ascertained when the long-awaited desideratum is attained and we know the composition of the less complex proteids.

The action of the enzymes, which is limited by the molecular constitution of the substances they hydrolyze and break up, is probably dependent upon their own constitution. Fischer's

researches seem to show that the molecular relations between the two are as intimate as those between a key and the lock whose wards it must fit before the position of the parts can be altered.   If this proves to be true, we shall look for a better understanding of the processes of digestion with the further extension of the stereochemistry of the nitrogen compounds.

Again a knowledge of the physiological action of definite radicals, which may differ according to their position in the molecule, is being reached by determining the effect of the introduction of a certain radical or of a change in its position. The ability to alter chemical structure at will by known reactions puts it into our power to ascertain how each change affects the protoplasm.   Thus in the phenols, a series of compounds allied to the tertiary alcohols, of which the so-called "carbolic acid" is a familiar example, True and Hunkel find that the introduction of the nitro group (NO_2) or the methyl group (CH_3) into the benzene nucleus increases the poisonous effect, while an increase in the *number* of hydroxyl groups (OH) or nitro groups (NO_2) has little or no effect.

PHYSIOLOGICAL MORPHOLOGY.

Within the past decade attention has been especially directed to the causes which affect the development of plants and determine both form and structure.   A moment's thought suffices to impress upon any one the fact that a great number and extreme variety of external agents are acting and interacting in most complex fashion upon all plants.   Some of the more obvious of these groups of external causes are even popularly recognized.   Thus one hears it said that poor soil and scanty water is the cause of the dwarfing of plants which, under better conditions, attain a greater stature.

Such apparently obvious deductions may be correct or may not be, but satisfactory and accurate analyses of the effects of external agents is a problem of the utmost difficulty, because it is well nigh impossible to alter experimentally one condition without really altering others at the same time.   The solution of the problems of physiological morphology, therefore, is to be attained only by the most assiduous care in experiment and induction.

Morphology.—In illustration of these problems I may refer to the recent studies made by Klebs on the external factors which control various reproductive processes among the algæ. By experimental analysis he has sought to determine the bearing of light, temperature, density of medium, and various other agents upon the production of zoospores and gametes. These studies have shown that it is possible to call forth a definite and very complicated physiological process, of far-reaching consequences, by appropriate changes in the environment. How they operate remains yet to be explained.

In the higher plants the investigations of Goebel and many others have shown the possibility of controlling growth and development in a similar way and to a remarkable extent. The relation between the different members of a plant has also been exploited, largely within the past decade, although its beginnings were long ago. The study of correlations has cast much light upon the causes of form, and has made more impressive than ever the wonderful plasticity of plants.

Correlations.—Qualitative correlations, particularly, offer an inviting though difficult field for investigation. I need only mention a few examples of such correlations. Upon the removal of the terminal shoot of a pine, one or more of the lateral shoots erect themselves and undergo appropriate changes in mode of internal growth and development, acquiring radial structure instead of dorsiventral, and branching on all sides instead of on the flanks. The transformation of sporophylls to foliage leaves following the removal of normal foliage has been a long-known example, to which renewed attention has been directed by the fine illustration of such change obtained in the experiments of Professor George F. Atkinson. It was shown by Knight, nearly a century ago, that the subterranean shoots of the potato, upon removal of the aerial parts, rise above ground and develop ordinary foliage leaves and flowers instead of tubers ; while, conversely, the enclosing of · aerial shoots in a dark chamber with saturated air gave occasion for the development of tubers, a phenomenon which is not uncommon under other than experimental conditions. A large number of similar transformations are now known.

Besides the accumulation of a greater range of such phe-

nomena, we must look to the future for a luminous theory of this reciprocal influence of organs. At present there is little that is satisfactory in the discussion of the nature of correlation. In what conceivable way can the removal of one member act upon other parts so as to alter the course of their normal development? What can be the nature of the stimulus which overcomes the diageotropism of the horizontal subterranean branches of the potato and induces upright growth and the development of foliage?

Regarding the quantitative correlations we are quite as much in the dark ; perhaps more so, because of the bearing of other functions. It is now clear that the greatly enlarged leaves and stems which develop after decapitation of a tree are in some way due to the increased food supply. But in what relation does the supply of food stand to these growths? Is the extensive removal of parts alone the stimulus which determines the revival of dormant buds and the formation of adventitious buds? Or does the increased amount of food act as the stimulus? But our present view of the movement of foods is that it is due to removal from solution, at the point where they are being used, of the substances which are needed. The using of food, indeed, is looked upon as both actuating and regulating in large measure the movement of food to any point. How then, consonant with these ideas, can a superfluity of food occur at any point, there to act as a stimulus? Or how can excess of food, occurring in any way, determine the increased *use* of food and so accelerate the growth of parts?

Pathology.—Closely connected with the study of the normal activities of plants are disturbances in the rate and character of function which are properly included under the term pathology. During the past decade very rapid advance has been made in a study of those pathological changes which are due to the presence of a foreign organism. Indeed, the phrase "diseases of plants" calls to mind almost exclusively the effects of parasites, which cause wilting by mechanical stoppage of water supply, extraordinary growths in the form of tumors, destruction of the chlorophyll to the detriment of photosynthesis, and a host of other evident changes. Indeed, as compared with other fields, we are tempted to say that this has

been over-cultivated. The difficulty, however, is not so much in over-investigation as in over-publication regarding the distribution of the diseases and the application of palliatives and remedies. This is justified, in a measure, by the enormous economic value of the crops attacked. But one cannot help wishing that the staffs of our experiment stations particularly would give greater attention to investigations on the nature of diseased conditions and less to repeating again and again the study of remedial operations.

There is thus one phase of pathology which has yet been comparatively neglected. The presence of a definite organism, whose activities clash with those of the host to the injury or death of the latter, is in itself an incitement to investigation. But we need also knowledge of those disturbed functions whose causes are dependent on other stimuli than the presence of a parasite. Some of these are doubtless internal and may long remain obscure, even as the causes of the so-called " spontaneous " movements have hitherto eluded observation. But unquestionably many plant diseases are due to untoward conditions of the environment, working sometimes through chemical, sometimes through mechanical, sometimes through ethereal, stimuli. This sort of work has been vigorously undertaken by the Division of Vegetable Physiology and Pathology at Washington, with full consciousness of the fact that, in order to attain results of value, there must be a fuller and more accurate knowledge of the normal processes.

At this point we are confronted by the difficulty of determining what processes are normal and what are pathological. It is the old question of sanity and insanity in a new guise—a question which each is tempted to answer in the same way as the old Quaker, who remarked to his wife: " Wife, they're all daft but thee and me; yea, and sometimes I think *thee* seems a little queer." What action shall be chosen as a norm is a matter of judgment, the general vigor of the plant alone serving as an imperfect criterion; imperfect, because we do not always know what constitutes vigor. Thus the full study of pathology needs not only the examination of the parasitic diseases, but also a wide acquaintance with the proper activities of healthy plants in order to determine what derangements are

produced in them by untoward circumstances and obscurer internal causes. In the latter is an almost unworked field which promises rich reward for patient investigation ; and that not only for the sake of pure science, but also for applied physiology as well.

If parasitic diseases cause among cultivated plants a loss of millions annually, is it unlikely that factors which can be controlled, if it is worth while to do it, cause in our crops a shortage whose money value may be many fold greater ? There are already practical experiments tending to show that most of our field and garden crops steadily suffer for want of water, a want which windmills and water-driven electric pumps might supply, often to great profit. We may not guess, we must *know* by experiments on a·large scale whether or not it will pay to supply water and to control other unfavorable conditions, before we dare recommend such measures to a practical world.

IRRITABILITY.

I must now turn to a topic which, while deeply involved in those that I have already discussed, deserves also special mention. I mean the relation of irritability to the well-being of plants. Seventeen years ago Sachs wrote : '' Irritability is universal in the vegetable kingdom. Vegetable life without irritability is just as inconceivable as animal life without irritability. Irritability is the great distinguishing characteristic of living organisms ; the dead organism is dead simply because it has lost its irritability.''

It would be impossible to state the case more strongly. But it is one thing for him who has conceived a truth to state it clearly, and quite another thing to have this truth enter into thinking and experimenting of investigators. Long after the clear enunciation of the importance of irritability by the great physiologist—the father of modern plant physiology,—too many were finding the chief rôle of irritability in those reactions which by deforming the body moved the connected parts. Plant movements, especially those due to changes of turgor, were long looked upon as the main evidence of irritability in plants. This conception was reflected in the text-books of the older day and still survives in many of the more elementary works.

After the bearing of irritability on movements was firmly established, it came to be seen that the regulation of the rate of growth and its resumption by certain parts which had ceased to grow was accomplished through irritability. Growth therefore, as well as movement, had important relations to irritability. But during the past decade, particularly, a better conception has been taking possession of physiological students. It is now perceived that *all* protoplasmic functions are initiated or controlled by external physical or chemical agents. This point of view is reflected in the masterly treatise of Pfeffer—his recent *Pflanzenphysiologie*. Throughout the first volume, discussing the physical and chemical phenomena connected with metabolism, the ability of the protoplasm to regulate its own operations and to control even the physical changes in adjacent parts is everywhere presented and insisted upon.

The idea of a stimulus, instead of being confined, as it once was, to the action of light, heat, gravity, and moisture, has now been greatly extended. Any external or internal change, slight or profound, gradual or sudden, which calls forth a corresponding change in the living protoplasm, is to be looked upon as a stimulus. The responses to stimuli, too, once thought of largely as those visible in curvature of motor organs or growing parts, are now conceived as of great variety. Invisible reactions probably outnumber the observable ones. Those producing a change of bodily form must be relatively few as compared with those which influence the performance of function or the course of development.

Diverse and numerous as are the stimuli which act upon plants, any conception of their operation would be faulty which fails to take into account the fact that stimuli of many unlike kinds and of unequal intensity are *interacting* to bring about the peculiar form and behavior of each individual plant. Think of the external agents which are known to be acting upon an ordinary land plant. About the aerial part the temperature varies from season to season, in our temperate zone changing from 30° below zero to 50° above ; it varies from month to month and from day to day, even from hour to hour. The light differs in intensity and direction from day to night and from hour to hour. It changes in its actinic effect, as the pho-

tographer well knows, in the course of a few minutes, a variation, by the way, whose effect on plants has been entirely unstudied as yet. The moisture in the air is hardly the same for any two consecutive days; the plant is deluged with water for some hours or days and dry between rains; it is enveloped in fogs and mists, wet with dews at night, and all but blistered by the sun during the day. Its subterranean part is surrounded by solutions whose amounts and composition are probably varying hourly; whose concentration and consequent dissociation is changing from time to time. The temperature of the soil is scarcely the same from hour to hour, between day and night, from day to day, and from season to season. Imagine now the numberless combinations possible among these varying factors, and remember that all these are interacting as stimuli upon the protoplasm. What wonder, then, that no two plants are alike; that Capsella may flower at 5 cm. height with a few minute entire leaves, or may grow ten times higher with abundant foliage and long racemes of fruitful flowers.

This different conception of irritability and its relations to the functions of the plant has led to many fruitful investigations during the past decade. The ingenious applications of plaster jackets for mechanical restraint of growth has thrown light not only upon the mechanical forces which can be exerted by growing organs, but casts a side light upon the difficult problem of the mechanics of growth. Researches upon the mechanics of curvature induced in growing organs by stimuli have been made by several observers, without obtaining, however, the concordant results which are to be desired. The subject, therefore, requires further study.

A satisfactory hypothesis as to what happens when an irritable organ is stimulated is still a desideratum. Is irritable protoplasm merely in a state of extraordinary lability, and does the stimulus initiate the decomposition of the protoplasm or of some unstable substance which it has produced? If this is true the metabolism of irritable organs which have been strongly stimulated ought to be different from that of a similar but quiescent organ, and different products may be expected. One of the most noteworthy advances in this direction seems to be the discovery by Czapek (unfortunately we have had as yet

only a preliminary paper) that roots after being geotropically stimulated contain notable amounts of reducing substances as compared with unstimulated roots, which, on the contrary, contain oxidizing substances.

Again, the transmission of impulses in plant tissues has been under frequent study. Haberlandt's seemingly well-founded conclusions regarding the transmission of impulses in Mimosa have proved untenable in the light of MacDougal's experiments, which also seem to shut out the possibility of the action of living protoplasm. The traveling of an impulse through a zone of dead cells is so marvelous that we are tempted to discredit the evidence of our senses; but that it occurs cannot be doubted. Thus again the discordant results of competent observers compels us to say that as good as nothing is now known.

ECOLOGY.

Within the past decade what may be considered a new division of plant physiology has been organized and has entered upon a development whose future extent and importance cannot yet be fully estimated.

Like every apparently new departure it is an evolution from the old. Though its rise has been phenomenal, many of its facts and principles have long been known. At the meeting of the Madison Botanical Congress of 1893, the word *ecology* was almost new to American ears, and doubtless some present at that Congress were surprised at the introduction of a resolution on so unimportant a subject. The adoption of a name and preferable form of spelling for the new science, however, has been very useful in unifying the practice of American writers, and is a good illustration of the beneficial effect of a formal agreement on a matter of usage.

In the last century the relations of plants to insects were studied and Christian Conrad Sprengel's *Entdeckte Geheimniss der Natur* was a pioneer work in this subject. Sprengel's work was destined to be forgotten for many years, and the further study of these interesting adaptations for the pollination of plants by insects was only revived by the prolonged observations and ingenious experiments of Charles Darwin. Since his time the work has been taken up vigorously and

knowledge enormously extended by Müller, Ludwig, Delpino, McLeod, Robertson, and a host of others.

The controlling influence of soil and climate upon the distribution of plants was also recognized and measurably understood long ago. In the classical works upon geographical distribution such as Grisebach's *Vegetation der Erde*, and Drude's *Pflanzengeographie*, the main features which form the basis for the grouping of plants are found to be those which constitute climate. Thus the moisture and heat relations of plants have dominated our thinking. The importance of these factors has not sufficiently impressed itself upon students of local distribution. Again and again in the past half century local lists of plants have been compiled with little reference to the true conditions which determine the growth of plants. The limits of these local floras have been the political boundaries rather than the natural barriers to plant migration or the physical features which determine climate. It has been the edge of the county, the boundary of the state, the limits of the country, which have been chiefly considered. In later years, however, the recognition of natural boundaries has become more common in these lists, and more attempts have been made to study the flora of a certain valley, a river system, and a table land. Even so, however, natural barriers have been looked upon as controlling plant distribution merely through their effect upon climate, to the neglect of other factors.

In the last decade the increasing attention which has been given to the effect of external agents of all kinds upon plants, and the growing appreciation of the effect of stimuli upon the plant form, acting through universal irritability, has led to the consideration of all the causes, small as well as great, which influence the well-being of plants. This knowledge, gradually accumulated, was first organized by Warming in his epoch-making work upon plant associations. Thus the subject of ecology was launched. The appearance of this great work not only brought into connection facts concerning the relations of plants to one another ; it cast a new light upon the subject of plant geography. Facts and statistics which before had been dull and uninteresting to many, because without philosophy, now became luminous with new meaning.

This new light upon the geography of plants comes not merely from a consideration of the effect of the great factors of light, heat, moisture, and soil structure upon the plant ; for these had been in a measure understood before. The new meaning arises from the introduction into the problem of the many minor factors of environment, which act as stimuli and of the interminable variety of combinations which these present in their influence upon plant welfare. Among these environing conditions none is of greater importance than the effect of plants upon one another, partly direct and partly indirect, befriending some neighbors and injuring others. Because of these relations there arise groups of plants which grow well together and others which are so antagonistic that they fly from one another's presence. These groupings may be due to causes the most remote, or to relations the most intimate; according as they are due to one or the other will the association be close or distant, the group large or small.

This phase of ecology, the study of plant societies, is yet in a somewhat chaotic condition. Not all the materials which are at hand have been satisfactorily organized, and much remains for future research. We await with impatience the settlement of various questions as to interpretation, and the acquisition of the multitude of new facts which are necessary before any true picture of the causes of form and the distribution of plant life is attainable.

It is a matter of some national pride that ecological investigations have been taken up vigorously by students in our own country and that from the new standpoint some valuable researches on plant distribution have already been made, even though a good share of so-called ecology is still rather crude. It is perhaps also a matter of local pride that the most extensive study has been made in one of our great western states, whose flora has been as yet comparatively little altered by the most potent of all disturbing factors, the hand of man. The *Phytogeography of Nebraska*, published a year or two ago by Pound and Clements, is the first extended study on plant geography in this country along distinctively ecological lines. The care and completeness with which their investigation was made render it a good example for future students of our flora;

yet one which doubtless succeeding contributions will improve upon as the subject becomes better organized. As other examples of similar study may be mentioned the paper of Professor Macmillan upon the more restricted flora of the Lake of the Woods, and the only partially published work of Dr. Cowles upon the flora of the Lake Michigan dunes.

Plant names.—I venture to say that one of the most significant results of the study of ecology and physiological morphology is the growing dissatisfaction which its students feel with present methods of nomenclature, or perhaps I ought to say classification. I do not refer to the large grouping of plants into families, orders, and divisions, but to the grouping of individual plants into species. This dissatisfaction is finding its expression among taxonomists as well. On the establishment of new species, we are hearing almost daily the plea that it is better to separate into many species a group of nearly allied forms, although the differences used to distinguish them be very much slighter than those heretofore used for species. That is, it is better to do violence to our old idea of a species, than to group together forms which in the field are easily recognized as unlike. This simply means that collectors and systematists are recognizing more fully the differences produced by unlike environment. It is a matter of common remark that the differences between individual plants, recognized as belonging to one species, are often greater than those which are used to separate species. Domesticated plants so easily pass into a variety of form that for the sake of maintaining a rigid idea of specific rank, cultivated plants have been quietly ignored. Now we are coming to see that in nature, as in cultivation, the plant is so plastic an organism that it is almost impossible to group together any individuals except those growing under identical conditions.

What has been devised as a convenience, namely, the establishment and naming of a species, is coming to be more and more of doubtful utility.

I will not undertake to say how much this species idea and nomenclature has retarded the true view of plant plasticity, but I feel sure that a good case might be made out for such a thesis. Whether any scheme can be devised which can replace

the binomial nomenclature, whether any better method can be used by naturalists for designating the organisms which they are studying, is a matter for the future. I venture, however, to prophesy that the present system of nomenclature, by which I do not mean any particular kind of practice, whether of Paris, or Berlin, or Kew, or Cambridge, or Rochester, but the fundamental method of naming plants itself, *must go.* Our mere judgments, which we call species, foisted upon plants, do not conduce to a clear understanding of vegetable phenomena, but rather blind our eyes to a recognition of otherwise obvious truths. Some other method of identifying plants must be devised.

CYTOLOGY.

There is yet one other field whose development I must not fail to mention though it does not pertain wholly to plant physiology. It goes without saying that the functions of the plant body resolve themselves into the functions of the unit of that body. In every organ, however simple or however complex, we recognize the individual protoplast as the unit of work as well as the unit of structure. Each, enclosed in the armor-like wall which it has formed for itself, though hampered in its movements, is able to carry on the chemical and physical processes which constitute life without notable hindrance. Within the protoplast, for which Sachs uses the expressive though unnecessary word energid, there go on certain changes which can be observed with the microscope. These changes we look upon as the index of the invisible ones whose significance we seek to understand. It is natural, therefore, that the closest scrutiny should be made of the observable changes which take place within the cell. This minute study began in the attempt to ascertain how the living protoplasm constructed the wall with which it jackets itself. Everydifference in composition which involved an optical alteration in the transmission of light and so became visible, has been studied with the utmost care.

Later, attention was attracted to the division of the various independent protoplasmic organs within the cell body. Some of these have been found to be relatively simple. The division of the nucleus, however, has shown a complexity and at the

same time a regularity which has challenged the minutest investigation and has made it the center of the greatest interest. So complex a series of changes, recurring with such regularity, argue an importance for both function and phylogeny which have made students eager to discover their secret. Therefore, within the last few years the behavior of the nucleus and of its different parts has been under study in all groups of plants with an exactitude never before dreamed of. Thus cytology has come to be an almost independent line of investigation. It is to be feared, however, that in many cases its exaltation has led students to mistake its real purpose and to consider it an end in itself. The visible processes within the cell will have little meaning unless they are looked upon as the mere index of its work. Unless the details of mitosis, for instance, are interpreted in the light of function or phylogeny they will certainly be misinterpreted, or will be meaningless. It is becoming a question whether we have not overestimated the importance of slight differences in nuclear phenomena and whether further knowledge can be expected from a study of the visible processes within it. At the same time decided progress is to be hoped for in a more intimate chemical knowledge of the substances composing the nucleus, as to their chemical constitution and their relation to chemical reagents, such as stains and fixing fluids, rather than in repeated counting of chromosomes and multiplied observation of the details of prophase and anaphase.

I have now discussed the chief features of plant physiology in which notable progress has been making during the last decade. The great advances in plant chemics and physics, the progress in the investigation of causes of plant form, the widening ideas of the property of irritability, the investigation of the social relations of plants, and the minute study of cell action, in spite of their diversity have one great end in view. This is nothing less than the solution of the great problem— the fundamental problem—of plant physiology, as of animal physiology. The secret which we must discover, the dark recess toward which we must focus all the light that can be obtained from every source, is the constitution of living matter. Entrenched within the apparently impregnable fortress

of molecular structure, this secret lies hid.   The attacks upon
it from the direction of physical chemistry and physiological
morphology, of irritability, of ecology, and of cytology, are ·
the concentrated attacks of various divisions of an army upon
a citadel, some of whose outer defenses have already been cap-
tured.   The innumerable observations are devised along par-
allel lines of approach, and each division of the army is creep-
ing closer and closer to the inner defenses which yet resist all
the attacks and hide the long-sought truth.   We see yet no
breach in the citadel.   Here and there we seem to approach
more closely and at certain points are getting glimpses, through
this loophole or that, of inner truths, hidden before.

One outer circle of defenses yet remains untaken, and until
that falls, it would seem that there is little hope of capturing
the inner citadel.   We *must* know more of the constitution of
dead substances, chemically related to the living ones.   When
the students of chemistry can put physiologists into posses-
sion of the facts regarding dead proteids, we shall renew the
attacks more directly, with greater vigor and greater hope of
success.

That ultimate success is to crown our efforts there is little
reason to doubt.   Ten years ago we little dreamed of the tre-
mendous strides since made toward the interpreting of life's
central truth.   The success of the past is the best augury for
the future.   The brilliant researches upon the chemistry of
carbon compounds inspire us with renewed hope and put into
our hands almost daily new weapons.

It is not possible to say to-day that life and death are only a
difference in the chemical and physical behavior of certain com-
pounds.   It is safe to say that the future is likely to justify
such an assertion.   In the meanwhile, we press forward along
the whole line.   Botany is more than ever full of meaning be-
cause with its sister sciences, it is no longer seeking things,
but the reasons for things.

PAPERS READ.

THE FERTILIZATION OF ALBUGO BLITI. BY F. L. STEVENS, Chicago, Ill.

The paper presents the results of two years' research on the development of the sex organs and the act of fertilization which, in this species, differs from the current conception of a fertilization in that the oosphere is a compound one, having about 100 functional nuclei. Each of these fuses with one male pronucleus derived from the antheridium. The development of these nuclei, and the organs that bear them is followed and the mitoses described, as is also the opening of the antheridial tube, and the fusion of the nuclei. A new cell organ, present during oogenesis, the coenocentrum, is described, and the ripening of the oospore followed.

THE EMBRYO SAC OF LEUCOCRINUM MONTANUM. BY FRANCIS RAMALEY, Boulder, Colorado.

The embryo sac of Leucocrinum is of the usual Liliaceous type. The sac is never greatly elongated but generally rather spherical. The polar nuclei fuse before the fecundation of the egg. The definitive nucleus moves from the center of the sac toward the posterior end before any division takes place. The synergids are large; they persist for a short time after the fecundation of the egg. The antipodal cells do not increase in number but a fragmentation of the nuclei sometimes occurs. The antipodals do not become completely disorganized for a long time and may still be recognized after a considerable mass of endosperm has been built up and the sac completely filled. The author found nothing to suggest a fusion between the definite nucleus and a male cell.

NOTES ON SUBTERRANEAN ORGANS. BY A. S. HITCHCOCK, Manhattan, Kansas.

A classification of the underground parts of perennial plants, especially

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the herbs, is made as below; accompanied also by notes and examples:
  Roots which form adventitious buds;
  Fleshy roots with a crown at apex;
  Crown with tap-root with fibrous roots;
  Rhizomes; simple, crown-bearing,
various subdivisions of each of above, with examples; the notes referred to plants in the vicinity of Manhattan, Kansas, confined chiefly to dicotyledons.

Some Monstrosities in Spikelets of Eragrostis and Setaria with Their Meaning. By W. J. Beal, Agricultural College, Mich.

A few plants of *Eragrostis major* made a second growth of some of the spikelets—more than twice the usual length, in a damp late autumn. A few spikes of *Setaria viridis* in same autumn had bristles bearing spikelets at the top, and one with a spikelet on the side of a bristle.

Studies of the Vegetation of the High Nebraska Plains. By Charles Edwin Bessey, Lincoln, Nebraska.

The physical conditions on the high plains of Western Nebraska include a general elevation of 1000 to 1200 meters above sea-level, a rainfall of but 40 centimeters per year, a very high isolation, a sandy soil, with a generally undulating surface, with now and then a shallow moist valley. Until recently these plains were swept annually with prairie fires.

The ecological conditions are taken up for the Box Butte plains where the grassy covering is an Agropyron-Stipa-Bouteloua formation; for the Snake Creek Valley, with a Sporobolus formation, surrounded by a zone of Distichlis; for the undulating surface with its exclusive carex formations; for rocky hills with a broad zone of Artemisia, in one series capped with a zone of Mentzelia; for the river bottom (Platte) with its Distichlis-Atriplex-Chenopodium formation.

The Tamarack Swamp in Ohio. By A. D. Selby, Wooster, Ohio.

A preliminary study of the Larix plant company as occurring in Ohio. The location of these bogs in the northeast countries extending as far south as Canton, and in the extreme northwest of Ohio, was pointed out; and a preliminary list of 86 species collected in these swamps by the author and E. W. Vickers, of Ellsworth, O., was presented in summary form. The rarer of these are of the genera *Sarracenia, Drosera, Trientalis*

Salix, *Arethusa*, *Coptis*, *Chiogenes*, *Ilicioides*, *Cornus* (*C. canadensis*), and species of *Viburnum*.

THE BREEDING OF FRUITS FOR THE NORTHWEST PLAINS. BY WM. SAUNDERS, Ottawa, Canada.

The author refers to the many failures which have followed the testing of a large number of the hardiest forms of useful apples on the northwest plains—these failures have led to the belief that the most hopeful line of work in future is the improvement of two species of Wild Crabs from Northern Siberia, *viz.*, *Pyrus baccata* and *P. prunifolia* both of which have been tested and found quite hardy. These have been crossed, but are very small, with hardy forms of the larger apples and some particulars of the results obtained from these crosses are presented.

FIELD EXPERIMENTS WITH "NITRAGIN" AND OTHER GERM FERTILIZERS. BY BYRON D. HALSTED, New Brunswick, N. J.

The study of leguminous root tubercles is uppermost in the minds of botanists and there is a practical side that deeply interests the cropgrowers. It has been shown that the microscopic symbionts greatly assist in the acquiring of nitrogen by the plants with which they live.

Professors Knobbe and Hiltner, of Tharand, Saxony, foremost in the study of the symbiotic germs, have produced pure cultures and these are placed upon the market as bottled lymph under the trade name of "Nitragin." With this "Nitragin," experiments are now in progress at the New Jersey Experiment Station, and some of the results are as follows :

The germs from five species, namely, *Vicia sativa*, *V. villosa*, *Trifolium pratense*, *T. repens*, and *T. incarnatum* were used, each in separate rows and upon the seeds of all five of the above-named legumes. Plants were lifted August 3rd, and the tubercles counted upon five plants and the average taken. It was found that the tubercles were more numerous upon the check plants than where the "Nitragin" had been used, and it seems evident, judging from the number of galls, that the germ fertilizer has had no appreciable effect.

A duplicate of the above trial was made upon soil where peas had been grown for four successive crops. Here the only difference to be noted was the large increase in the number of tubercles, those of the old pea land being nearly double those upon new land.

A still more extended experiment was made with thirteen leguminous crops upon land that had been variously treated in previous years with soil remedies for club root in turnips. It was found that sulphur and lime both materially diminished the number of tubercles.

An equal area was given to a test of the germ fertilizer offered through the trade under the name of "Alinit" and recommended for crops generally. The actual weights of five leguminous crops and four cereals (one failed) showed a grand total in favor of the check, although the difference was only slight.

Experiments with several other substances that might be supposed to stimulate the development of germs in the soil indicate that they have no wholesome influence.

THE DURATION OF BACTERIAL EXISTENCE UNDER TRIAL ENVIRONMENTS. BY HENRY L. BOLLEY, Agricultural College, N. D.

The paper is based upon studies made from ordinary cultures which had been preserved for a number of years. Many had been allowed to become air-dried, suffering the varying condition of the laboratory atmosphere. Other cultures had been hermetically sealed and thus kept in fresh form. The results are of interest because of the longevity shown for many of the germs; and because of the indicated possibility of keeping typical cultures in normal form as to gross characters and as to the morphology of the individual germs for long periods of time.

SUGGESTIONS FOR A MORE SATISFACTORY CLASSIFICATION OF THE PLEUROCARPOUS MOSSES. BY A. J. GROUT, Brooklyn, N. Y.

It is axiomatic that the classification of plants having an alternation of generations should be based on both gametophyte and sporophyte characters.

Schimper and nearly all modern authors except Lindberg and Braithwaite give undue weight to sporophyte characters as in the *Isotheciæ* where a heterogeneous collection of plants is put in the same subfamily because of their sporophyte characters alone. Lindberg, while classifying more scientifically, often overestimates single characters, *e. g.*, when he puts *Porotrichum* (*Thamnium*) with the *Neckeraceæ* because of its leaf characters, all its other characters indicating a close relationship to the *Hypneæ*.

There are two characters of the pleurocarpous mosses whose importance in classification is generally underestimated: The presence or absence of a central strand in the stem and the presence and degree of development of fine transverse lines on the lower dorsal plates of the teeth of the peristome. The latter are present and well developed in the subfamilies *Hypneæ* and *Brachytheciæ*. Also in the genera *Isothecium*, *Porotrichum*, *Pterogonium*, and *Leseuraæ*, which are closely related and con-

stitute a separate subfamily differing from the above-mentioned sub-families in leaf structure.

These lines are as well developed in *Thuidium* and allied genera of the *Leskeaceæ* (as usually constituted), as in the *Hypneæ* and if taken in connection with the perfect Hypnaceous peristome indicates that these forms are at least as closely related to the *Hypneæ* as to the *Leskeaceæ*. In the latter family these lines are vestigial and the whole peristome degenerate so that *Thuidium* must either be an intermediate form or a separate derivative from the Hypnaceous type. The presence of a perfect peristome with these lines well developed in the *Pterygophyllaceæ* indicates that this family is closely related to the *Hypneæ*.

The entire absence of these lines in the *Fontinalaceæ*, *Neckeraceæ*, and *Climacium* taken in connection with other characters indicates that these forms constitute a group by themselves, coordinate with the forms previously mentioned and possibly derived independently from the acrocarpous mosses. The anomalies of the *Fabroniaceæ* may possibly indicate a third similar group.

The central strand is the physiological homologue of the vascular bundle and for many reasons would seem to be a far more important character than the length and shape of the capsule; yet in our present systems it is given far less weight. The presence of a central strand is usually correlated with the presence of the costa in the leaves except in aquatic or subaquatic species. This indicates that *Amblystegium* and *Plagiothecium* are not naturally grouped and must also modify the present systems in many cases.

The author wishes it distinctly understood that the above statements are thrown out as suggestions because his knowledge of forms is far too incomplete to warrant any final statements.

NOTES CONCERNING THE STUDY OF LICHEN DISTRIBUTION IN THE UPPER MISSISSIPPI VALLEY. BY BRUCE FINK, Fayette, Iowa.

Brief statement as to area covered in the report and of work already accomplished. Present lack of completeness of the survey in many portions of the territory under consideration as to species collected, and as to data concerning habitat, frequency of occurrence and ecologic factors in general. Statement as to number of species now known in the region and probable number to be added by future study. Suggestions as to ecologic factors which should receive attention in future work, with citation of literature which shows a beginning of such work. Paper closes with a list of the species known within the area as based upon the literature cited in something over thirty titles and specimens examined by the writer for various collectors and not previously recorded. The list is arranged to show distribution of each species by states, which ones are addi-

tions to distribution within the area since Tuckerman wrote, and which are added to North America since that time.

BOTANICAL TEACHING IN SECONDARY SCHOOLS. BY W. C. STEVENS, Lawrence, Kansas.

BOTANICAL TEACHING IN SECONDARY SCHOOLS. BY IDA CLENDENIN, Brooklyn, N. Y.

ON THE OCCURRENCE OF THE BLACK ROT OF CABBAGE IN EUROPE. BY H. A. HARDING, Geneva, N. Y.

During the season of 1898 this disease was observed by the author on cabbage and related plants in fields near Haarlem in Holland, Bonn, Karlsruhe, Fulda, Berlin, Halle on Saale, and Kiel in Germany, Slagelse in Denmark, Zurich in Switzerland, and Versailles in France.

Wherever an opportunity to visit a cabbage field presented itself the disease was always found, although with the exception of Switzerland and possibly Denmark it did not appear to be of economic importance.

Field observations were supplemented whenever possible by microscopic and cultural examinations.

Sections of infested plant parts presented the same characters as is shown by the disease common in America.

Cultures uniformly produced a predominant growth of yellow colonies, agreeing in general appearance and in morphology with *B. campestris* Pam.

Subcultures were brought to New York and inoculated into cabbage and cauliflower. In the case of germs obtained from Zurich, Switzerland, the inoculation invariably produced a disease exactly like that found common in our fields and behaved in all respects like cultures obtained from diseased plants in Wisconsin and New York.

With germs brought from other points in Europe the results were not so conclusive.

A THOUSAND MILES FOR A FERN. BY CHARLES EDWIN BESSEY.

Giving an account of the finding of the Southern Maiden Hair Fern

(*Adiantum capillus-veneris*) in the Black Hills of South Dakota, August 24, 1898, and giving some idea of the vegetative conditions intervening between Lincoln, Nebr., on the southeast and Cascade, the station where the ferns were found. They grow along the banks of a warm stream (25° C.) which issues from numerous large springs. The ferns are certainly indigenous. Specimens were secured for distribution to the larger herbaria.

A SUMMARY OF OUR KNOWLEDGE OF THE FIG WITH RECENT OBSERVATIONS. BY WALTER T. SWINGLE, Washington, D. C.

A summary of existing knowledge concerning the fig, caprifig, and caprification including the results of recent observations by the author in North Africa, Greece, and Asia Minor. The opinion is reached that the fig and caprifig represent the sexes of a dioicious species. The successful shipment of caprifigs with contained insects to California is also described, together with suggestions for the naturalization of the caprifig and fig insects in such manner as to place the cultivation of the Smyrna fig in the United States on a commercial basis.

THE CLASSIFICATION OF BOTANICAL PUBLICATIONS. BY WILLIAM TRELEASE, St. Louis, Mo.

The subject considered is that of a purely botanical library which may be supposed to stand in the closest possible connection with collections of works referring to all other branches of knowledge; *i. e.*, with a general library—and is therefore a contribution to the general discussion of the proposed international catalogue of scientific literature. The scheme of topics would be stated as follows :

1. Works of miscellaneous contents, but of botanical interest, and treatises on several branches of botany.

2. Biographies of botanists, and collected writings of miscellaneous contents, whether purely botanical or botanical in part only.

3. Nomenclature, taxonomy, and descriptive botany.

4. Morphology and organography.

5. Vegetable physiology, including ecology.

6. Vegetable pathology, including the injuries of plants, and therapy.

7. Evolution, natural selection, etc.

8. Man's influence on plants, artificial selection, etc.

9. Physiography, floras, etc.

1. As above.

1.1 General treatises containing more or less matter of botanical interest.

1.11 Publications of societies, colleges, museums, etc.

1.111 Botanical gardens, parks, etc.

1.12 Journals (except when restricted to some single branch of botany, these three classified geographically).

1.13 Text-books, lecture outlines, etc. (those restricted to special subjects would be sought under such subjects).

1.2 Dictionaries and encyclopedias.

1.21 Language dictionaries.

1.22 Encyclopedias, technical dictionaries, etc.

1.23 Nomenclators, dictionaries of plant names, and purely botanical encyclopedias.

1.24 Bibliographic aids of general contents.

1.25 Indexes to illustrations, exsiccata, etc.

1.3 Icones.

1.4 Popular and economic botany.

1.41 Botany of literature.

1.42 General and miscellaneous economic botany.

1.421 Botany of agriculture.

1.422 Botany of horticulture.

1.4221 Fruits.

1.4222 Vegetables.

1.4223 Decorative plants.

1.423 Botany of forestry.

1.4231 Deudrologies, sylvas, etc. (local floras would be consulted here).

1.42311 Winter (and other seasonal) manuals.

1.4232 Anatomical classification of woody plants.

1.42321 Strength and properties of timber.

1.424 Botany of pharmacy, food adulteration, etc.

1.4241 Poisons and toxicology.

1.4242 Mechanical effects of vegetable substances.

1.4243 Histological pharmacognosy.

The paper outlines similarly other topics of the scheme.

The Geotropism of the Hypocotyl of Cucurbita. By Edwin Bingham Copeland.

Experiments show that the plant executes the geotropic response without direct regard to the consequences, and without the power of adaptation to unusual conditions. In nature the rapid growth of the under side of a prostrate hypocotyl bears the cotyledons upward, but if a young plant be placed horizontal with the cotyledons fast and the roots free, the same response bears the roots upward, and is therefore likely to be immediately fatal. While the object of geotropism is to secure a certain arrangement

of the *longitudinal* elements of the plant,—root, hypocotyl, cotyledons,— the stimulus is a disturbance of the normal disposition of the *transverse* pressure of the tissues. It is not necessary for the perception of a geo- tropic stimulus that the plant compare the difference in position or pres- sure of its two halves ; for if the plant is laid prostrate the lower half will of itself grow more rapidly than the upper, as may be demonstrated by cutting the halves entirely apart.

THE DESTRUCTION OF CHLOROPHYL BY OXIDIZING ENZYMES. BY A. F. WOODS, Washington, D. C.

This paper details results of experiments going to show that the mosaic disease of tobacco is due to oxidizing enzymes rather than to a " living fluid contagium" as suggested by Beijerinck. It also shows that these enzymes are unusually abundant in many other cases of variegation and in the diseases known as peach-yellow and peach rosette ; and in these cases also ascribes the destruction of the chlorophyl to the abnormal abundance of these ferments.

THE EFFECT OF HYDROCYANIC ACID GAS UPON THE GERMINATION OF SEEDS. BY C. O. TOWNSEND, College Park, Md.

The use of hydrocyanic acid gas in the destruction of insect pests in stored grains has led the writer to investigate the effect of this poisonous gas upon the germination of seeds. In the experiments that form the basis of this paper seeds in both the dry and damp state, have been tested with differ- ent strengths of gas and for different periods of time. In the case of dry grains and seeds it was found that they might remain for several weeks in an atmosphere of hydrocyanic acid gas many times as strong as is required for the almost instantaneous destruction of insect life without appreciably injuring their germinative power. Indeed the gas under these conditions slightly accelerates germination and the subsequent rate of growth of the seedlings is slightly above the normal.

Seeds that have been soaked in water become very sensitive to the pres- ence of hydrocyanic acid gas. If the seeds have been soaked for twenty- four hours they cannot germinate if more than 0.030 of a gram of potas- sium cyanide per cubic foot is used in generating the gas. Even 0.003 gram of potassium cyanide per cubic foot has a very marked effect upon the time of germination of seeds that have been soaked in water for twenty-four hours. If the grains and seeds have been soaked but six hours they are more resistant than when soaked for a longer period but even under these circumstances germination is distinctly retarded by the pres- ence of hydrocyanic acid gas.

Some Physiological Effects of Hydrocyanic Acid Gas upon Plants. By W. G. Johnson, College Park, Md.

A brief report of the first precise experiments with hydrocyanic acid gas upon young fruit trees, both dormant and in full foliage.

Etiolative Reactions of Sarracenia and Oxalis. By Wm. B. Stewart, Minneapolis, Minn.

An anatomical examination of etiolated leaves of Sarracenia and Oxalis shows the increase of length of supporting tissues by increase in size and multiplication of cells, and the non-development of portions which functionate in light only. Etiolative reactions are almost purely adaptive in their nature.

The Mycorhiza of Tipularia. By Julia B. Clifford, Minneapolis, Minn.

The roots of Tipularia show some marked specializations of structure for adjustment to the presence of an endotropic fungus with which a symbiosis is formed. The fungus is differentiated into a vegetative mycelium, with external absorbent branches, and internal branches serving as organs of interchange.

Cultures of Uredineæ in 1899. By J. C. Arthur, Lafayette, Ind.

Successful cultures of eleven species of *Uredineæ* were made upon their host plants, showing the connection of æcidial and teleutosporic stages. The following is a list of the associated forms, the host plants, and nature of the cultures.

1. *Puccinia convolvulus* Cast. on *Convolvulus sepium* L, and *Æcidium calystegiæ* Desm. on same host, with sowings of teleutospores.

2. *Puccinia Phragmitis* (Schum.) Koern. on *Phragmites communis* Trin. and *Æcidium rubellum* Pers. on *Rumex crispus* L. and R. *obtusifolius* L., with sowings of teleutospores.

3. *Puccinia americana* Lagh. on *Andropogon scoparius* Mx. and *Æcidium pentstemonis* Schw.* on *Pentstemon pubescens* Sol. with sowings of æcidiospores and teleutospores.

4. *Puccinia windsoriæ* Schw. on *Triodia cuprea* Jacq. and *Æcidium pteleæ* B. and C. on *Ptelea trifoliata* L., with sowings of æcidiospores.

5. *Puccinia Vilfæ* A. and H. on *Sporobolus asper* (Mx.) Kunth. and *Æcidium verbenicola* K. and S., with sowings of æcidiospores.

6. *Puccinia peridermiospora* (E. and T.) Arth. on *Spartina cynosuroides* (L.) Willd. and *Æcidium fraxini* Schw. on *Fraxinus viridis* Mx., with sowings of teleutospores.

7. *Puccinia caricis* (Schum.) Reb. on *Carex stricta* Lam. and *Æcidium urticæ* Schum. on *Urtica gracilis* Ait., with sowings of æcidiospores.

8. *Puccinia angustata* Pk. on *Scirpus atrovirens* Muhl. and *Æcidium lycopi* Ger. on *Lycopus sinuatus* Ell., with sowings of æcidiospores.

9. *Uromyces euphorbiæ* C. and P. on *Euphorbia nutans* Lag. and *Æcidium euphorbiæ* Am. Auct. on same host, with sowings of æcidiospores.

10. *Phragmidium speciosum* Fr. on *Rosa humilis* Marsh. and *Cæoma miniata* Am. Auct. on *Rosa sp.*, with sowings of teleutospores.

11. *Triphragmium ulmariæ*—on *Ulmaria rubra* Hill and *Cæoma ulmariæ*—on same host with sowings of æcidiospores and uredospores.

THE EMBRYOLOGY OF VAILLANTIA HISPIDA. BY FRANCIS E. LLOYD, New York.

The archesporium consists of about twelve cells. But one of the megaspores produced therefrom normally becomes the embryo-sac, the development of which follows in much the usual fashion in a position, however, removed from the archesporium ; this position is attained by a migration of the megaspore involved out of the nucellus into the micropylar canal. Fusion of the polar nuclei takes place at some distance from the egg, toward which, however, the endosperm moves and to which it ultimately becomes closely applied. The antipodals are three, one of which is very long, one end being plunged into the disintegrating archesporium, which is believed to serve as food. The embryo has a suspensor which forms outgrowths into the endosperm, these acting as food-absorbing organs. The endosperm enlarges at the expense of the integument which has the appearance of a tissue undergoing digestion. A part of the integument remains as a seed envelope. The reserve food consists of cellulose and starch.

THE DIVISION OF THE MEGASPORE OF ERYTHRONIUM. BY JOHN H. SCHAFFNER, Columbus, O.

Our knowledge of the process of reduction is still very fragmentary,

and the observations and interpretations presented by the several investigators differ widely. *Erythronium albidum* and *E. americanum* present favorable objects for the study of the important phenomena which take place during the transition from the sporophyte to the gametophyte. As is the case in the lilies generally, the megaspore of Erythronium arises from the archesporial cell, directly, by differentiation and not by division. The archesporial cell can usually be distinguished before the first of October, and it continues to develop until after the first of December, when it passes into a partial resting stage and does not complete its division until early the next spring. The cell, therefore, in which the reduction takes place has a period of development extending over six months.

In the fall, while the nucleus is expanding, the chromatin network begins to thicken until a continuous band is formed. In the spring the band twists itself up into twelve loops, which break apart and form twelve very large, coiled chromosomes. The chromatin granules never appear very distinct, and they do not begin to divide until the chromatin band begins to form the loops. After the pseudo-reduction the chromosomes are arranged on the spindle threads with their closed ends turned outward and are then gradually untwisted and pulled apart at the middle. This results in the transverse division of each chromosome, one transverse half going to each daughter nucleus. The division of the megaspore of Erythronium is therefore essentially the same as in *Lilium philadelphicum*, and it seems to the writer that a transverse, qualitative division is the only interpretation possible.

ARE THE TREES ADVANCING OR RETREATING UPON THE NEBRASKA PLAINS? BY CHARLES EDWIN BESSEY.

Two years ago the writer presented facts to show that the Rocky Mountain Pine (*Pinus ponderosa scopulorum*) is slowly advancing eastward in certain localities upon the foothills of Western Nebraska. Further studies confirm this conclusion. In the present paper facts are set forth which show that where fires and other adverse agencies are kept out, the deciduous trees of Eastern Nebraska are moving westward, up the water-courses, and up the hills adjoining them.

THE FLORA OF FRANKLIN COUNTY, OHIO. BY A. D. SELBY, Wooster, O.

A comparison of the known flora of Columbus (Ohio) with that listed in the catalogue of Wm. S. Sullivant, in 1840, in tabular form as given below. It shows a gain of the known list amounting to 353 species, of which 117 are introduced. In other words, 184 species of the present known flora, or 16.7 per cent. of the present are of introduced species.

TABLE SHOWING A SUMMARY OF KNOWN SPECIES AND VARIETIES OF
PLANTS INTRODUCED AND INDIGENOUS IN FRANKLIN COUNTY
(OHIO) FLORA.

Orders.	Sullivant, 1840.		Selby, 1899		Total
	Int'd.	Native.	Int'd.	Native.	
Ophioglossaceae - - -		2		5	5
Filices - - - -		23		26	26
Equisetaceae - - - -		3		3	3
Lycopodiaceae - - -		1		1	1
Total Pteridophytes - -		29		35 ·	35
Pinaceae - - - -		1		3	3
Taxaceae - - - -				1 ·	1
Total Gymnosperms -		1		4	4
Typhaceae - - - -		1		1	1
Sparganiaceae - - -		1 ·		2	2
Naiadaceae - - - -		2		5	5
Scheuchzeriaceae - -		2		2	2
Alismaceae - - - -		3		6	6
Vallisneriaceae - - -		1		1	1
Gramineae - - - -	14	51	23	21	94
Cyperaceae - - - -		70		91	91
Araceae - - - -		4		4	4
Lemnaceae - - - -		1		3	3
Commelinaceae, - - -		1		1	1
Pontederiaceae - - -		1		1	1
Juncaceae - - - -		5		9	9
Liliaceae - - - -		22	2	25	27
Smilaceae - - - -		2		3	3
Amaryllidaceae - - -		1		1	1
Dioscoreaceae - - -		1		1	1
Iridaceae - - - -		3		4	4
Orchidaceae - - - -		16		21	21
Total Monocotyls - -	14	188	25	252	277
Saururaceae - - . - -		1		1	1
Juglandaceae - - -		7		8	8
Salicaceae - - - -		4	3	14	17
Betulaceae - - - -		3		3	3
Fagaceae - - - -		8		10	10
Ulmaceae - - - -		3		4	4
Moraceae - - - -		1		1	1
Urticaceae - - - -	3	4	3	5	8
Santalaceae - - - -		1		1	1
Aristolochiaceae - - -		2		3	3
Polygonaceae - - -	6	12	8	16	24
Chenopodiaceae - - -	3	1	6	1	7
Amaranthaceae - - -	1	1	4	3	7
Phytolaccaceae - - -		1		1	1
Aizoaceae - - - -	1		1		1

Orders.	Sullivant, 1840.		Selby, 1899.		Total.
	Int'd.	Native.	Int'd.	Native.	
Portulacaceae - - -	1	1	1	1	2
Caryophyllaceae - - -	4	9	7	10	17
Nymphaeaceae - - -		1		2	2
Ceratophyllaceae - - -		1		1	1
Magnoliaceae - - -				2	2
Anonaceae - - - -		1		1	1
Ranunculaceae - - -		23	5	27	32
Berberidaceae - - -		3		3	3
Menispermaceae - -		1		1	1
Lauraceae - - - -		2		2	2
Papaveraceae - - -		4	1	5	6
Cruciferae - - - -	4	14	14	17	31
Capparidaceae - - -			1	1	2
Crassulaceae - - - -		2	1	2	3
Saxifragaceae - - -		8	1	10	11
Hamamelidaceae - - -		1		1	1
Platanaceae. - - -		1		1	1
Rosaceae - - - -	1	27	6	37	43
Leguminosae (Incl.) - -	2	29	11	38	49
Geraniaceae - - - -		1	1	2	3
Oxalidaceae - - -		2		5	5
Linaceae - - - -			1	2	3
Rutaceae - - - -		2		2	2
Sinarubaceae - - - -			1		1
Polygalaceae - - -		2		3	3
Euphorbiaceae - - -	1	5	3	7	10
Limnanthaceae - - -		1		1	1
Anacardiaceae - - -		5		5	5
Ilicaceae - - - -		1		1	1
Celastraceae - - - -		4		4	4
Staphyleaceae - - -		1		1	1
Aceraceae - - - -		3	1	4	5
Hippocastanaceae - -		2		1	1
Balsaminaceae - - -		2		2	2
Rhamnaceae - - -		2		2	2
Vitaceae - - - -		3		5	5
Tiliaceae - - - -		1		1	1
Malvaceae - - - -	3	2	5	3	8
Hypericaceae - - -	1	4	1	6	7
Violaceae - - - -	1	6	1	10	11
Passifloraceae(?) - -		1(?)		1(?)	1(?)
Thymelaceae - - -				1	1
Lythraceae - - - -		4		4	4
Onagraceae - - - -		7	1	8	9
Araliaceae - - - -		3		3	3
Umbelliferae - - - -		19	2	20	22
Cornaceae - - - -		3		8	8
Pyrolaceae - - - -		1		2	2
Monotropaceae - - -		1		2	2
Ericaceae (Incl.) - - -		1		2	2
Primulaceae - - -		8	1	8	9
Ebenaceae - - - -		1		1	1

Orders.	Sullivant, 1840.		Selby, 1899.		Total
	Int'd.	Native.	Int'd.	Native.	
Oleaceae - - - -		3	1	4	5
Gentianaceae (Incl.) - -		5		7	7
Apocynaceae - - -		2		2	2
Asclepiadaceae - - -		6		8	8
Convolulaceae - - -		3	1	4	5
Cuscutaceae - - - -		1		1	1
Polemoniaceae - - -		5		6	6
Hydrophyllaceae - - -		6		6	6
Boraginaceae - - -	3	4	4	8	12
Verbenaceae - - - -		3	1	4	5
Labiatae - - - -	4	28	11	36	47
Solanaceae - - - -	2	2	6	5	11
Scrophulariaceae - -	3	24	6	25	31
Lentibulariaceae - - -		1		1	1
Orobanchaceae - - -		2		3	3
Bignoniaceae - - - -		1	1	1	2
Martyniaceae - - -	1		1	⋌	1
Acanthaceae - - - -		3		3	3
Phrymaceae - - -		1		1	1
Plantaginaceae - - -	1	2	2	3	5
Rubiaceae - - - -		8	1	12	13
Caprifoliaceae - - -		6	2	9	11
Valerianaceae - - -		4		6	6
Dipsaceae - - - -	1		1		1
Cucurbitaceae - - -		2		2	2
Campanulaceae - - -		8		8	8
Compositae (Incl.) - -	7	80	30	142	172
Total Dicotyls - - -	54	485	159	667	826
Total, all classes - -	68	703	184	958	1142

THE FUNGUS INFESTATION OF AGRICULTURAL SOILS IN THE UNITED STATES. BY ERWIN F. SMITH, Washington, D. C.

A continuation of studies begun by the writer in 1894 on the parasitic soil Fusaria of the U. S. Results are detailed of completed experiments on soil infections with the watermelon fungus, over 500 of which have been obtained. It shows that related species are likely to prove equally destructive to plants of other families; e. g., cabbage, tomato, sweet potato. The fact to be specially emphasized is that these fungi live in the soil over winter and attack the plant from the earth. Further, a soil once infected with one of these resistant fungi becomes worthless for growing the agricultural plants subject to it, for a long series of years, and consequently the greatest care should be taken to avoid the spread of these parasites to land which is now free from them.

Useful Trees and Shrubs for the Northwest Plains of Canada. By Wm. Saunders, Ottawa, Canada.

In this paper is given the results of a large number of experiments conducted during the past eleven years in testing the hardness and usefulness of many species and varieties of trees and shrubs both native and foreign on the Canadian Experimental Farms in Manitoba and the Northwest Territories. Some particulars are given as to the success which has attended this work and attention called to some of the groups to which the hardiest forms belong; references are also made to many individual species and varieties which have been found most useful.

The Occurrence of Calcium Oxalate and Lignin during the Differentiation of the Buds of Prunus Americana. By H. L. Bolley and L. R. Waldron, Agricultural College, N. D.

The paper consists of a short résumé of the occurrence of crystals of calcium oxalate and of the presence of lignified tissues as observed by Mr. Lawrence Waldron in a study conducted upon the development or life history of the buds of *Prunus americana*. It was found that the crystals of calcium oxalate occur in quite surprising abundance in the meristematic tissues of the bud and in the very youngest stages of the scales of the bud, and that the oxalate becomes lessened in proportionate quantity as the tissues develop. Lignification of the hairs and scales of the bud commences at a very early period of their development. While it is usually assumed that calcium oxalate is a waste product of metabolism its occurrence in such large quantities in the meristematic cells of the bud and scales would seem to indicate a question as to whether it has not a definite value at this point at this particular time in the life history of the plant.

Two Diseases of Juniperus. By Hermann von Schrenk, St. Louis, Mo.

The species of Juniperus are trees which have few fungous and insect enemies. The author describes two destructive diseases of *Juniperus virginiana*, one of which is also found in *Juniperus bermudiana* and *Thuja occidentalis*. The first one is due to an undescribed species of *Polyporus*. Large holes are formed in the heartwood of the trunk, one above the other. Each is full of mycelium and has a thick white lining. consisting of wood fibers from which the lignin had been removed, leaving the pure cellulose. The fruiting part forms on the outside of the trunk, forming around a dead branch. It has been reported so far from Kentucky and Tennessee. The second form of destruction is more widely

spread. It is due to a *Polyporus* probably *P. carneus.* Long pockets are formed in the heartwood of a tree, filled with a brown, brittle wood, which has characteristic properties. The sporophore forms in the branch holes on the trunk. They have a flesh-colored hymenium and are quite common. Attention is called to the fact that a very large per cent. of the individuals of *Juniperus virginiana* are defective because of one or other of these fungi.

THE CRYSTALS IN DATURA STRAMONIUM L. BY HENRY KRAEMER, Philadelphia.

The value of the study of calcium oxalate in plants from a systematic standpoint, having been demonstrated by R. von Wettstein in an exhaustive study of the members of the Umbelliferae, the author has undertaken a study of some of the genera of Solanaceae and herewith presents the examination of the crystals of *Datura stramonium.* In this plant are to be found all the forms of crystals of calcium oxalate which are present in the various members of the group Solanaceae.

In the root are found numerous cryptocrystalline crystals which are more or less deltoid in shape and about 2 to 10 μ in their largest diameter. These crystals have been described by Vesque as being "sable tetraédrique" but inasmuch as calcium oxalate crystals are only known to occur in the monoclinic and tetragonal systems, it seems that they are hemihedral forms of either one or the other of these systems. It is not unlikely but that farther investigations will demonstrate that they are hemihedral forms of the monoclinic system. In the stem we find the same kind of crystals as well as a similar distribution of them that were noted in the root. There may be present, however, a few monoclinic prisms, raphides or rosette aggregates of crystals. In the petiole of the leaf we find numerous hemihedral cryptocrystalline crystals ($20 \times 10 \times 10 \mu$) besides many monoclinic prisms ($10 \times 10 \times 14 \mu$) or pyramids ($10 \times 10 \times 25 \mu$) or rosette aggregates of crystals (30μ in diameter). In some specimens the cryptocrystalline crystals may be largely replaced by small monoclinic prisms, pyramids, and acicular crystals.

In the nerves of the leaf lamina while there are still numerous cryptocrystalline crystals there are also large pyramids ($28 \times 15 \times \mu$) and rosette aggregates (28μ). In the mesophyl of the leaf the cryptocrystalline crystals are entirely replaced by rosette aggregates (17 to 24μ in diameter), occurring in each parenchyma cell directly below the palisade layer. Rosette aggregates are also found in flower stalk, calyx and corolla. In the filaments of stamen occur very small cryptocrystalline crystals. In the walls of the ovary of the flowers the cells containing cryptocrystalline crystals ($2 \times 1 \times 1 \mu$) are very numerous and only a few of the cells possess rosette aggregates (14μ).

It is rather interesting to note that the cryptocrystalline crystals found

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in such abundance in the root and stem are replaced in part in the petiole and nerves of the leaves by prisms, pyramids, and rosette aggregates and that in the lamina of the leaf the prisms and pyramids unite to form rosette-shaped aggregates only.

The fact that we find monoclinic prisms in Hyoscyamus and raphides sometimes in Atropa and Solanum further indicates that the crystals in *Datura stramonium* are of the monoclinic system. That there is a close relationship between the different genera of the North American Solanaceæ is evidenced by the observations that some of the parenchyma cells of some of the elements of nearly all of the genera and species appear to contain the hemihedral cryptocrystalline crystals, these being present in the greatest amount in the genera of the Solaneæ and Datureæ.

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ANTHROPOLOGY.

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ADDRESS

BY

THOMAS WILSON

VICE PRESIDENT, AND CHAIRMAN OF SECTION H.

THE BEGINNINGS OF THE SCIENCE OF PREHISTORIC ANTHROPOLOGY.

DENMARK.

Scientific investigation into prehistoric anthropology began in Denmark in the early part of the present century. Its start was more the result of accident than design. The King of Denmark provided, in 1806, for a scientific investigation of his country, corresponding in some degree with the aggregate duties of what in our country are the Geological Survey, the Natural History Division of our National Museum and Department of Agriculture, and the Bureau of Ethnology.

DOLMENS.

Almost the first obstacle the committee met and which, being unable to explain, caused it to put on its studying-cap and led to an extensive discussion, was a dolmen, one of the common and now well-known prehistoric burial places. Associated with this discovery, were the stone hatchets, both polished and chipped for polishing, also now so well-known. The studies of the historian and archaeologist failed utterly in assigning these to any period or people known in historic times. The ancient Sagas were studied in detail, but never developed an age of culture wherein axes other than those of iron were used. As the Commission's investigations were extended, the number of these objects, both dolmens and axes, were increased and other implements were added to the list.

Denmark kept the lead in her interest in the discoveries relating to prehistoric man, and in the formation of the new science which was to become Prehistoric Anthropology.

Another Commission was formed, composed of Professors Forschhammer, Steenstrup and Worsaae, the latter of whom was the special representative of the Science of Archaeology, though the other two would perhaps have been equally great in archaeology had they not already been celebrated by their earlier work in biology and geology. Worsaae's labors as an archaeologist were overshadowed by his subsequent greatness as a statesman ; he became one of the Cabinet Ministers of the Kingdom, and died in office.

This Committee continued the investigations into the new science by the discovery of the shell mounds. That at Havelse was first and became the representative specimen, but it was soon found that shell mounds or deposits existed along the coast in every direction, and what had theretofore been supposed to be the natural surface of the land was really the result of human labor and the evidence of human occupation. The farmers and land owners in their respective neighborhoods had already discovered that these mounds were not composed of the usual sand and clay, but mostly shells, which, in a state of great decay, were more or less mingled with black soil ; and they had carted away much of the material to be distributed over the surface of their fields for enrichment.

An investigation, commenced at Havelse, showed not only the artificial character of these shell mounds, but the presence of many pieces of stone, principally flint, which had been broken in such way as to indicate human intervention and an adaptation to human use. These objects ran pretty nearly the entire range of prehistoric implements as we now know them : hammerstones, axes, hatchets, flakes, scrapers, arrowpoints, spearheads, knives, spindlewhorls, gouges, crescents, daggers, etc., etc. There were also objects of shell, horn, and bone, and many fragments of pottery.

The more important implements from certain deposits were found to be of stone, with a piercing point or a cutting edge,

mostly chipped into shape, though some had been pecked or battered and then ground or polished.

In other deposits objects of different material were found, and among the rest the presence of bronze implements was detected.

The number and kind of these implements, their methods of burial or deposit, with the associated objects, enabled the archaeologist to assign to them a chronological sequence; first in epochs of culture, and second in improvement made within these groups.

These epochs of culture divided themselves according to the material employed for cutting implements, into the ages of stone, bronze, and iron. This was the first step in the establishment of the new science of Prehistoric Anthropology. The Royal Danish Museum of Antiquities was established in 1816 now occupying the Princessen Palace, at Copenhagen. It was to be the home of the archaeological collections of the Kingdom, and here Mr. Thomson with the aid of Professors Forschhammer and Steenstrup classified, arranged and displayed the objects found, and here the new science was born.

LAKE DWELLINGS.

In 1853 and 1854 the waters of the Swiss lakes were, from natural causes, reduced to a low stage, and Dr. Ferdinand Keller embraced the opportunity to investigate certain peculiarities which, reported to him by the fishermen and builders on the water's edge, had excited his curiosity. One of these was that certain localities with a sloping shore apparently well suited for drawing the seine, were rendered useless for this kind of fishing because of obstructions, believed to be the decayed stumps of a submerged forest covering the bottom, catching the lead-line, the lifting of which allowed the fish to escape. It appeared that excavations had been made as for a building, in the year 1829 during another period of comparatively low water, when piles and other objects of antiquity, believed to have been Roman, had been found. Being thus satisfactorily accounted for, their discoverers gave them no further heed, and the objects were not brought to the attention of, nor submitted to, the inspection of any antiquarian. Dr. Keller's

first surprise was at the number of these stumps, the similarity of their appearance, and the regularity with which they had grown. His surprise was increased when, on lifting one out from its bed, it was found *not* to be a stump, but a sharpened and pointed pile bearing evidence of human workmanship, which had been driven into the ground. A cursory examination showed this to be the condition of all. This was evidence of a previous human occupation; and as the late discovery in Denmark began to have its effect upon the mind of Keller, it became apparent that these were the evidences of a human occupation of the Swiss lakes at some prehistoric period. This ripened into a certainty when it was discovered that like conditions existed in other places, not only in Zurich, but in other lakes. Reports of these, from both Switzerland and Denmark, spread over western Europe and naturally excited the interest and curiosity of thinking men, especially those of France and England. The objects themselves were passed about, and descriptions with illustrations were brought under the eyes of the people of these countries, who turned their attention to similar known objects of their own countries theretofore unrecognized. Like the discoveries in Denmark and Switzerland, the great interest centered in the similarity of the respective implements in the various classes found in these widely separated countries. If I recur to this question of the similarity of implements found in different countries, it is because of its importance. It formed the foundation of the science. It was by reason of this similarity that the Scandinavian discoverers and early students were able to determine the prehistoric ages. By comparison of implements of their own countries with those of Scandinavia, and a recognition of the similarity between them, the students from other countries of western Europe were enabled to correlate and identify the culture of the prehistoric man; and this knowledge finally crystallized into the universal recognition of the three prehistoric ages of stone, bronze, and iron. The prehistoric man had but few kinds of implements : the hammer or maul, the hatchet, the knife, scraper, arrowpoint and spearhead, spindle-whorl, points of bone and horn, objects, principally ornaments, of shell and pottery. These implements were substantially the

same in every locality, so far as concerned the Neolithic period. The polished stone hatchets were identical, whether found in the dolmens of France and England, the dolmens and shell heaps of Scandinavia, the lake dwellings of Switzerland, or the terramare of Italy; and in after years, as our knowledge of the prehistoric world increased, this similarity was found to extend throughout the Eastern and Western Hemispheres, as well as the islands of the sea. The similarity was not confined to one class of implements, but included nearly all in each age. There were minor differences, but the implements could be recognized as the same wherever found. For example, the hatchets were long or short, had a head or poll, well finished or left rude, and were round, flat or square in section. Those square in section were from Scandinavia; short stumpy ones with unfinished poll, from the British islands; the poll pointed or rounded and well finished, from Continental Europe; the button-headed, from Britanny. But with all this, they were always the same implement. The material might differ with the locality, but otherwise, as to use and method of manufacture, they appeared the same. As investigation proceeded, this similarity of implement extended. The polished stone hatchet of America was found to be the same, with only the same differences of detail. Some from Illinois, made of flint, have a spreading edge, almost of a crescent form, the corners forming the points after the style of some of the battle axes of mediæval knights; those from Chiriqui are flattened on both sides by a sort of chamfer which makes them appear hexagonal in section. It has been argued that this similarity of implement was due to the similarity of human thought adapted to human necessity. The similarity of human thought and action may be admitted, although it necessarily has its limitations.

The similarity of the implements found among different peoples widely separated, is not accounted for by the theory of human thought and human needs. The classification of prehistoric culture into ages of stone, bronze and iron was based on the similarity of the implements of each age found in the respective countries, and this was the result of migration, communication or contact between the peoples.

DISCOVERIES OF DARWIN AND BOUCHER DE PERTHES.

By the middle of the century, students of prehistoric matters of the Old World had about accepted the prehistoric ages of stone, bronze, and iron. Investigators sought to discover the man who had made and used these implements; and the few skulls that had been gathered in earlier times, the significance of which had not been understood, were subjected to reexamination in view of the new light upon prehistoric matters. Chief among these were the Canstadt skull, discovered in 1706, and the Engis skull, in 1822. In these studies the pathway of prehistoric science and knowledge was being slowly blazed when, in the year 1859, two great discoveries relating to the origin and antiquity of man were published which had something of the effect of an earthquake upon former scientific conclusions. One, the origin of the human species through evolution, by Darwin; and the other, the acceptance as artificial of the paleolithic implements found by M. Boucher de Perthes in the valley of the Somme. Boucher de Perthes had, as early as 1836, but seriously since 1841, been investigating the peculiarities of certain chipped flints found at Abbeville, France, and as far south as Amiens, along the line of the canals and railroads then in course of construction. These he had recognized as the work of man, and claiming for them the highest antiquity, he asserted them to be antediluvian. His discovery was at first unfavorably received. In 1853, Dr. Rigollot announced his adhesion to the theory; in 1859 Dr. Falconer discovered the presence of the bones of *Elephas antiquus* at St. Acheul and their association, more or less intimate, with the chipped flint implements of Boucher de Perthes. In 1859 numerous geologists of England visited the locality, and some of them, especially Mr. Arthur J. Evans, now Curator of the Ashmolean Museum, Oxford, then a lad accompanying his father, Sir John Evans, had the good fortune to find one of the chipped flint implements *in situ*. There was much contention over the proposition connecting man with these implements, and there were many unbelievers. Some disputed the antiquity of the deposit, others the human manufacture of the implements and, curiously enough, the greatest opposition

came from the French geologists and the greatest support from
the English.

It is not here declared that the geological formation was not
early understood by eminent scientists who visited the locality,
but there does not appear to have been any publication *in ex-
tenso* of that formation and the strata of which it is composed
and the fauna found therein, until that of M. D'Ault Du Mes-
nil in the Revue Mensuelle de l'Ecole d'Anthrpologie (Sixi-
eme Année, IX, 1896), and of which I translated and pub-
lished the general portions in the American Antiquarian (Vol.
XXI, No. 3, 1899, pp. 137-145).

There were found to be several geologic and paleontologic
strata. In the lower layers the bones and teeth of *Elephas
meridionalis* were found associated with the *Rhinoceros
merkii;* in the middle strata the *Elephas primigenius* and the
Elephas antiquus were mingled ; while in the upper layers the
Elephas primigenius alone appeared. The implements in the
lower strata were large and rude, while in the upper they
became smaller and finer and better made, forming the type
called by M. D'Ault Du Mesnil St. Acheulléen. The surface layer
contained objects belonging to the later ages, and does not here
concern us. The discussions over the theory of the human
origin of these implements soon came to a close by its general
acceptance. There have been continuous and almost illimitable
discussions over details, but none over the general proposition.
"One swallow does not make a summer," and a single dis-
covery, either of an implement or a locality, is of slight value
in the establishment of any general proposition in prehistoric
anthropology. If the discovery of chipped flint implements
had been confined to those of Boucher de Perthes, they would
never have made any headway. But the attention of those
interested in the subject having been attracted to these chipped
flint implements, they, as the polished stone hatchets in the
Neolithic period had done previously, were found in greater or
less numbers in many localities throughout the principal
countries of western Europe. Then came a comparison of the
same implements from different localities, and it was decided
that they were related and formed a stage of culture so different
from that of polished stone as to show that they belonged

to another people, occupying the country at an earlier date. To this period Sir John Lubbock gave the name, Paleolithic.

These chipped flint implements were found by scores of investigators and searchers, in hundreds of places, to the number of tens of thousands.

As before remarked, it was the likeness or similarity of the implements, not only in general form, but in the details, as well as in their material, mode of manufacture, and possible method of use which clinched argument. They so closely resemble each other in their details as to show that the men who made and used them not only belonged all to the same stage of culture, but that either through migration or commerce they must have had intercommunication. They might or might not have been blood-relatives, but that they were acquaintances and taught each other the modes of manufacture of these implements, seemed to have been admitted on all hands.

The discoveries of the prehistoric ages of stone have been extended to Africa. Professor H. W. Haynes and General Pitt-Rivers in Egypt, Mr. Seton-Karr in Somaliland, have made discoveries of Paleolithic implements. Discoveries of Neolithic implements have been made by Mr. J. de Morgan in the valley of the Nile, and by a Belgian, in the valley of the Congo. All have been found in sufficient numbers to establish the fact that they were not isolated or sporadic specimens, but were evidence of an extensive human occupation of their locality.

DIFFERENCES BETWEEN PALEOLITHIC AND NEOLITHIC CULTURES.

In treating of the science of prehistoric anthropology, it is imperative that the differences between the culture of Paleolithic and Neolithic times should be noticed. Necessarily this must be confined to the Old World, as the discoveries in America have not been sufficient to establish the lines between the two periods.

Mons. Gabriel de Mortillet formulated the differences between his Madelainien epoch (the last of the Paleolithic period) and his Robenhausen epoch (the first of the Neolithic period), and has arranged them in parallel columns that they may make a graphic representation :

LATEST PALEOLITHIC.

(1) Climate cold and dry, with extreme temperatures.
(2) Existence of the last grand fossil species—the mammoth.
(3) Chamois, marmot, the wild goat, in the plains of France.
(4) Reindeer, saiga (antelope), elk, glutton, white bear, in the center of Europe.
(5) Hyena and the grand cat tribe.
(6) No domestic animals.
(7) Human type uniform.
(8) Population nomadic.
(9) Hunters and fishers, but no agriculture.
(10) Stone implements always chipped.
(11) No pottery.
(12) No monuments.
(13) No burials; no respect for the dead.
(14) No religious ideas.
(15) A profound and pure art sentiment.

EARLIEST NEOLITHIC.

(1) Climate and temperate uniform.
(2) The mammoth extinct.
(3) Chamois, marmot, and wild goat have gone to the summits of the mountains.
(4) These animals have emigrated toward the Arctic region.
(5) No hyenas or grand cats.
(6) Domestic animals abundant.
(7) Human type much varied.
(8) Population sedentary.
(9) Agriculture well developed.
(10) Stone implements polished.
(11) Pottery.
(12) Monuments: Dolmens and menhirs.
(13) Burial of the dead.
(14) Religious ideas well developed.
(15) Art sentiment crude, confined to geometric decoration.

The radical difference between the Paleolithic and Neolithic periods is that they were in different geologic epochs. The former belonged to the quaternary, the latter to the present, epoch. In the transition from the Paleolithic to the Neolithic the glaciers ceased, the climate became temperate and uniform, the animals peculiar to the earlier conditions passed away and others affected by the change in climate migrated. There were eighteen species of cold-loving animals in Western Europe during the Paleolithic period, which migrated to other localities because of the moderation of the temperature incident to the commencement of the Neolithic period. Thirteen of these migrated to cold countries by latitude going to the North, the reindeer, the musk-ox, the blue fox, etc.; five, like the chamois and mountain goat, migrated to cold countries by altitude, going up on the mountains.

Comparing the industries of the two periods, we will see some of those of the earlier continued into the later period, and some of those of the later were invented or improved.

The art of chipping stone into implements was continued from the earlier to the later, but to it was added the art of grinding and polishing. All our smoothed and polished stone implements and objects had their origin in this Neolithic culture. Sawing and drilling stone began here. The bow and arrow, the first projectile machinery in the world, here had its birth. The twisting of flaxen thread, weaving and the making of cloth clothing, commenced in this period. Pottery making was begun which, in itself, wrought a revolution in human culture. The earliest monuments of the world, the great mounds, tumuli, dolmens, menhirs, cromlechs, and the fine specimens of prehistoric architecture, date from this period.

The family was formed, and the clan or tribe organized with a local habitation and a name. Villages, and finally towns, were established ; animals were domesticated, flocks and herds with farms and pastures came into being ; agriculture increased the means of subsistence ; a division of labor became fixed, and mechanics with trades were partially inaugurated. Though the Neolithic man, from our point of view, was a savage, yet compared with his predecessor, the Paleolithic man, he had made a long stride towards civilization, whether from savagery to barbarism may be suggested but need not be decided, nor even argued here.

PALEOLITHIC IMPLEMENTS DESCRIBED.

The recognition of the artificial character of the chipped flint implements found by Boucher de Perthes and the many who came after him, and which gave an impetus to the science of Prehistoric Anthropology, made an opportunity, if it did not create a necessity, for some sort of classification. The Scandinavian classification of stone, bronze and iron had been accepted, but these late discoveries demonstrated an earlier period, and called for a subdivision of the age of stone.

All the implements found were of flint and chipped. During this period man did not know to rub one stone against another to make either of them smooth or sharp, as he did in the later age ; so the first was called the chipped stone age, and the other the polished stone age. Sir John Lubbock gave them the names, respectively, Paleolithic and Neolithic. These Paleo-

lithic implements of chipped flint being found mostly in the alluvial gravels, the name alluvial, alluvium (French), diluvial, diluvium (English?), were given respectively to them.

These implements require a description by which they can be recognized from those of other ages. They were all of flint or some chippable material, many of them were made from boulders or concretions. Some were so chipped as to leave the smooth part of the boulder as a grip for the hand. They varied in length from six or eight inches down to three, in width from five to two, and in thickness from three inches to one. They were generally almond-shaped and had a point or cutting edge at the small end; some of them, made from ledge-rock and not from boulders, were brought to an edge all round. In outline they resembled the leaf-shaped implements of later ages; but when viewed edgewise the difference was manifest in that these were much thicker. The thickness is usually about half their width; an implement four inches wide would be about two inches thick, and one two inches wide an inch thick, while leaf-shaped implements of that width would not be one-half as thick.

RIVER DRIFTS, VALLEYS, AND TERRACES.

A further explanation is as to the formation of the geologic deposit in which the implements were found, and so an indication as to their age. It is believed that at an earlier period in the geology of the country the water of the rivers on its way to the sea eroded the earth (as is shown by the geologic models, principally of the Rocky Mountains, in the United States National Museum), and formed valleys, making them to reach from one hill to the other and as deep as the present bottom of the rivers; at the second stage the water in the rivers, becoming less in volume and slower in movement, began the process which has been carried on from that day to the present in all river valleys, the cutting or washing of the river bank at or from one point or locality where the water ran swifter and stronger, and carrying it further down the stream where the water ran slower and weaker. In this manner the river terraces were formed, each successive terrace (counting from the hill), is represented at present in many of our American rivers,

—especially the Ohio, where three terraces exist on either side of the stream. In the chronologic formation of these terraces, that nearest the hill was the oldest, that nearest the stream the latest. The bottom of each terrace was, naturally, laid down first and, consequently, was older than the top. So the bottom of the first terrace (nearest the hill) was the oldest, and the top of the terrace (that nearest the stream) was the latest.

These Paleolithic implements have been found in the bottom of the first terrace and, consequently, were a part of the earliest deposit. And as they continued throughout the various terraces and in the different parts of them, it is believed that the Paleolithic period in these localities began with the formation of the river-valleys and was coexistent with them.

During all this period no implements of less enduring material than flint have been found, if any ever existed. Nor have there ever been any human remains found in the river valleys; but there have not been found the remains of any animal so small as man or whose bones were so light and frail as are his.

DIFFERENCES IN CLIMATE.

No traces have been brought to light of either the habitation or the raiment of the man of this period. It has been suggested that he had no need for either. The climate was warm, moist, and rainy, he required neither dwelling nor raiment to keep him warm or dry, for, like the savages of warm climates generally, he may have preferred to run naked. This is regarded as entirely feasible in the climate then prevailing in western Europe.

But there came a change, supposed to be represented by the glacial epoch, when the climate became cold and wet, and man required protection and so was driven to the caverns for shelter. Here is found the first evidence of raiment. Thus began what has been called the cavern period.

EPOCHS OF THE CAVERN PERIOD.

Different classifications have been made and different names given to these. Some of the early scientists named them from the animals of the time and locality. Lartet named them respectively, Cave Bear, Mammoth, Reindeer, and Ox ; Dupont,

Mammoth, and Reindeer. The English generally employed the terms "river-drift" for the earlier Paleolithic and "cavern" for the later. De Mortillet made an exhaustive study and a consequent elaborate classification named for, and based on the

Time	Ages	Periods	Epochs
Quaternary Actual. / Historic. — Proto historic.	Iron	Merovingien	Wabenien (Waben, Pas-de-Calais)
		Roman	Champdolien (Champdolent, Seine-et-Oise)
			Lugdunien (Lyon, Rhone)
		Galatien	Beuvraysien (Mont Beuvray, Nievre)
			Marnien (Department of Marne)
			Halstattien (Hallstatt, Austria)
	Bronze	Tsiganien	Laraudien (Larnaud, Jura)
			Morgien (Morges, canton of Vaud, Switzerland)
	Stone	Neolithic	Robenhausien (Robenhausen, Zurich, Switzerland)
			Campignyen (Campigny, Seine-Inferieure)
			Tardenoisien (Fére-en-Tardenois, Aisne)
Quaternary Ancient. / Prehistoric.		Paleolithic	Tourassien (La Tourasse, Haute-Garonne) Ancient Hiatus
			Madelainien (La Madeleine, Dordogne)
			Solutréen (Solutré, Saône-et-Loire)
			Moustérien (Le Moustier, Dordogne)
			Acheuléen (Saint-Acheul, Somme)
			Chelléen (Chelles, Seine-et-Marne)
Tertiary.		Eolithic	Puycournien (Puy-Courny, Cantal)
			Thenaysien (Thenay, Loire-et-Cher)

industries found in certain localities: The Chelléen after Chelles (Seine-et-Marne), Acheuléen after St. Acheul (Somme), Mousterien after the cavern of Le Moustier (Dordogne), Solutréen after the station of Solutré (Saone-et-Loire), Madelenien after the rock-shelter of La Madeleine (Dordogne), and Tourassien after La Tourasse (Haute-Garonne), the last representing the hiatus between the Paleolithic and Neolithic ages. This classification was carried throughout the prehistoric ages. (See chart.)

Objection may be made to this nomenclature, but a slight experience will satisfy one of its excellence. Its principle is to give to an epoch of culture the name of a locality where that particular culture is manifested in its greatest purity. This may be an arbitrary system, but it has the great desideratum of all systems of nomenclature—certainty and definiteness. By it one knows exactly what is meant, and this is the chief purpose of nomenclature. The American geologic classification is based largely on the same system.

HIGH PLATEAU PALEOLITHS, IGHTHAM, KENT.

Among the many discoveries of Paleolithic implements in Europe was a certain class which indicated a human occupation earlier than those found in the river gravels. These belong to the high plateaux between the headwaters of the streams. The principal discovery of implements of this class was by Mr. Benjamin Harrison, of Ightham, Kent; but knowledge of the significance thereof is due to the great geologist, Prof. Joseph Prestwich.

A small stream runs past the town of Ightham where it joins the Medway. This stream has the usual terraces in its valley which, like other terraces, are formed of river drift. These valleys contained Paleolithic implements of the usual kind similar to those heretofore described. The theory was that the river valley had been eroded, the sand and gravel cut or washed away, then carried down the stream and deposited where the current became weaker; thus would be involved all the Paleolithic implements within the scope of the valleys or ravines that fell into it. The information furnished by Mr. Harrison's discovery was that, on the high plateau levels *not*

involved in the valleys or the ravines leading to it, the same kind of Paleolithic implements were found practically on the surface. The theory of Professor Prestwich, founded on Harrison's discovery, carries us back one step further in the chronology of Paleolithic man. He believed that the implements were made and used by man on these high plateaux before the commencement of the formation of the river-valleys ; that, being scattered over the surface where they had been left by their owners, they have remained, until now found, undisturbed, uninfluenced by the erosion, the which as it proceeded, cut away the sand and gravel and drew the other implements into the valley or into the general current which carried the sand and gravel down, and deposited the implements with the débris in the form of a terrace. These Harrison implements *not* being within the reach of this erosion, remained *in situ* and are now being found on the surface of the plateau above. Implements and even workshops indicated by the presence of certain tools and style of implements remained on the high plateaux and are there found the same as in other Paleolithic workshops. If they had been within the influence of the stream and had been carried down by its waters, they would have been found in the drift of the terrace below ; but not having been thus involved, they were not affected and so remained in their original places until now found. This conclusion, if correct, pushes the Paleolithic age one epoch farther into the past ; instead of the implements being found in the bottom of the river terrace at the completion of their journey, they are found on the high plateau which was originally, and for the others, the beginning of the journey.

TERTIARY MAN.

Another step in the science of prehistoric anthropology (whether forward or backward is yet to be determined) was the discovery of implements and objects of supposed human origin, or which bore a supposed artificial character, alleged to be evidence of man's existence in the tertiary geologic period. The first report in this direction was by Mons. J. Desnoyers who, on June 8, 1863, presented before the Academy of Sciences at Paris, certain fossil animal bones and pieces of wood from the

quarries of sand and gravel at Saint Prest, near Chartres, France, which were believed to belong to the pleistocene formation, the marks, imprints, and striæ on which were such as could have been made by man and were, therefore, said to be evidence pointing towards his existence in that period.

In 1867 the Second Congress of Archaeology and Prehistoric Anthropology met at Paris and was largely occupied over a presentation of, and discussion upon, the evidence of tertiary man. Mons. L'Abbé Bourgeois presented a series of flint objects which were so chipped or broken as to appear to have been done by man. Other objects were presented by various persons, all alleged to have a bearing upon the main question and tending to establish the existence of man in the tertiary period. These were of different materials: bones cut or marked, teeth or bones drilled, wood and bone carved or gnawed, etc., etc., until a rather extensive series of objects was gathered and which, if the contention of their finders had been successfully maintained, would have gone far toward the establishment of the existence of man in the tertiary period.

Professor Capellini found the fossil rib-bones of a whale in the tertiary deposit at Monte-Aperto, Italy. These ribs had evidently been cut with a sharp knife or tool, and might easily have been done by man. There was no attempt at engraving, only certain kerfs across the ribs. Prof. Capellini presented his discovery to the Academy of Lincei, at Rome, and before the Congresses of Archaeology and Prehistoric Anthropology at Budapest, in 1876, and at Paris, in 1878. I had the pleasure of examining these specimens in the Museum of the University of Bologna, and was much impressed with the contention of Professor Capellini.

Dr. Arturo Issel, one of the leading scientists of Genoa, joined the advocates of tertiary man before the International Congress of Archaeology and Prehistoric Anthropology in 1867, by the presentation of a human skeleton, or a portion of one, found at a depth of ten feet in the blue clay, said to have been of pliocene formation, near Savona, Italy. The skeleton was discovered by other persons and had been distributed and portions lost, so that only certain members came to Dr. Issel. There were no other animal bones found in the deposit, but

many fossil shells, which undoubtedly belonged to the plio-
cene. If the skeleton was contemporaneous with the original
deposit, it would be good evidence of the existence of man
during that period. Four human skeletons were found at
Castenedolo, Italy, by Professor Ragazzoni, then searching for
fossil shells. The deposit was determined to belong to the
pliocene, or at least to the tertiary.

There were, throughout Western Europe, perhaps a dozen
more instances of objects alleged to be human or related to
human, found in tertiary deposits. The principal of these,
and that which obtained the greatest prominence, was the dis-
covery of Abbé Bourgeois at Thenay, near Pontlevoy (Loire-
et-Cher). Among other reasons for the prominence of the
discovery of Abbé Bourgeois was the fact that the discovery
was near his residence, where he could give it much of his
personal attention ; and he was able to attend many or all of
the scientific meetings, whether of archaeology, geology, or
palaeontology, wherein the subject would find interested audi-
tors, with many opportunities for the presentation of the sub-
ject. From the year 1867, when his discovery was presented
to the International Congress of Archaeology and Prehistoric
Anthropology, at Paris, until 1883, before the Association
Française at Blois, he kept up an aggressive warfare. The
deposit at Thenay was agreed to belong to the tertiary, and it
had not been disturbed ; therefore, if the objects were made
by man, they would be evidence of his existence at the time
the deposit was made. They were all of flint and had evi-
dently been worked ; whether naturally or artificially was the
important question. Some had been crackled as though by
fire, and others had been chipped as though by man. I have
three of these pieces of flint in the Museum at Washington,
and am free to confess that, had they been found under con-
ditions ordinarily possible to prehistoric man, I would have no
hesitation in accepting them as artificial. The presentation of
these flint objects before the various archaeological congresses
created great interest and begat much discussion. At one,
that in Brussels, an International Committee of fifteen mem-
bers was appointed to investigate the question and make re-
port. The committee divided, as might have been expected.

Eight members were of opinion that the pieces of flint were artificially chipped : De Quatrefages, Capellini, Worsaæ, Englehardt, Augustus W. Franks, Valdemir Schmidt, D'Omalius, and Cartailhac;[1] five members were opposed: Virchow, Steenstrup, Desor, Neirynck, and Fraas ; Marquis de Vibray was favorable, but with reserve, and Van Beneden unable to decide.

It will thus be perceived that the question was difficult to determine, and much could be said on both sides. If the opposing forces of the learned men who, on the ground, marching in the presence of each other and of the objects themselves, and, as at Blois, with the deposit whence the objects came under their eyes, were still unable to determine the question, it would be venturesome for us to attempt it. Since the meeting at Blois there has been but little discussion of the flints from Thenay. It would seem as though neither party was convinced by the other, and both were content to maintain their former opinions and cease the discussion. Sir John Evans revived it after a fashion in his Presidential Address before the British Association, at Leeds, in 1890, wherein he took opposite grounds.

Discoveries similar to that of the Abbé Bourgeois were made by M. B. Rames, a distinguished geologist of Aurillac at a locality called Puy Courny, near · Aurillac ; by Charles Ribeiro, near Lisbon, Portugal ; and by Joseph Bellucci, of Perugia, at Otta, Monteredondo, Italy. They all fall into the same category and receive the same treatment. In the conclusion to be awarded to the existence of man during the tertiary period, they stand or fall together.

PITHECANTHROPUS—DUBOIS.

The presentation of this branch of my subject would be incomplete without a reference to the great discovery made by Dr. Dubois, at Tinil, Java. Dr. Dubois is an educated physician, a graduate of the Leyden University, interested in prehistoric anthropology, with a sufficient knowledge of geology and palaeontology to enable him to make satisfactory investigations in the field. He was attached to the Dutch Army as a

[1] Mons. Cartailhac changed his opinion, but not until several years afterwards.

medical officer, and with it sent to Java. He lived there for six years, and having found a deposit of fossil bones at Tinil, prosecuted his researches therein for three summers with great success. During this work he found certain portions of a skeleton which, if not human, was nearer to it than to any other. Dr. Dubois has published a preliminary report of his discovery containing a section and a plan of the field of his explorations, and photographic copies of the human (?) remains. When this publication appeared and fell into the hands of the physical anthropologists, whether of Europe or America, who, by their knowledge of human and comparative anatomy, were the best qualified to judge, they almost universally settled the question to their own satisfaction in the shortest and easiest way, by the decision that the remains were human and that Dr. Dubois had done nothing more than discover an ancient graveyard. There were few persons in the United States prepared to combat this view. Prof. O. C. Marsh visited Leyden in attendance upon the International Congress of Zoology, Sept., 1895, and upon his return announced that this was a much graver question than had before been recognized.

I had the gratification of visiting Dr. Dubois and seeing his collection. Like Prof. Marsh, I was amazed at the showing made. He had, in his laboratory, many thousand bones from the deposit at Tinil. They were all fossilized, their weight was greatly increased, and their color much darkened, while the human (?) bones had an identical appearance and it was evident that they came from the same deposit and were the same age. It is the accepted conclusion on every hand that the bones and deposits belonged to the tertiary period, but what particular epoch, I am not prepared to say.

The dilemma presented by the discovery of Dr. Dubois in relation to the antiquity of man is that, if the bones are really those of a human individual, it carries the antiquity of the human species back to the tertiary period. If the individual is not human, because the deposit of the tertiary period is too early, then he must have been the precursor of man and so the "missing link." This dilemma must be recognized and the conclusion made harmonious. Darwin would have accepted this as a representative specimen of his "missing link."

De Mortillet was of opinion that the animal that chipped the flints of Thenay was not man, but his precursor, which he named "Anthropopitheque," or "Anthropopithecus." Dr. Dubois has the same idea or theory with regard to the man of his discovery, and he has given it the name " Pithecanthropus erectus."

The discussion over tertiary man or man's precursor, remains in abeyance. Each of the two parties holds to his respective opinions, pro and con, and the question awaits further developments.

NEOLITHIC AND BRONZE AGES CONTINUOUS.

If there was a belief in a hiatus between the Paleolithic and the Neolithic ages of Europe, there was no belief in a hiatus between the Neolithic period and the age of Bronze. It seems conceded that there was no appreciable difference in the races of people in Western Europe in these two ages. It is also conceded that the stage of culture continued in both practically the same ; that all or most of the industries of the Neolithic period were continued into the Bronze age, subject, however, to the natural improvement which came with added experience. The difference between the two ages, then, was the increased facility in performing the function of civilization by reason of having cutting implements of bronze instead of those of stone. The making of bronze was evidently a human invention and has little or nothing to do with a difference in race, nor beyond the benefit or improvement made by the invention, has it much to do with a change in culture.

Copper was easily procured throughout Europe, and implements of that metal were made in Neolithic times and doubtless continued to be made in the Bronze age. But the advent of bronze was a totally different affair. Copper did not require casting ; it might have been hammered into the desired form and so made into implements ; but the knowledge of melting and casting was indispensable to the age of Bronze. Bronze is a mixture of copper and tin in the proportion of eight or nine parts of the former to one of the latter. The question, whence came the bronze which was so plentiful throughout Europe, has always been one of the problems of prehistoric archaeology.

The tin necessary for making bronze appears to have come from the country around the Straits of Malacca. The methods of its migration or transportation to Europe, whether the tin was brought over, whether it was melted, mixed with copper and then brought over, both being in the form of ingots, or whether it was cast into implements and then distributed, are facts absolutely unknown, and they probably will always remain so. Prehistoric bronze objects have been found in Southern Asia and throughout Europe. The excavations of Dr. Schliemann into the Hill of Hissarlik brought many of them to light. Foundries have been discovered in most European countries; in France nigh a hundred, the latest by Mons. Paul du Chatelier, in Brittany. The most extensive one yet found was that at Bologna, Italy. It contained the metal in all stages of preparation for casting, together with molds and crucibles ready for use. There were fourteen thousand (14,000) pieces of bronze, some in ingots but most of it in worn-out implements broken into small pieces suitable for the melting-pot.

Epochs of culture in the age of bronze have been manifested by improvements in style in the hatchets of southern Europe and the fibulæ of Scandinavia.

PHYSICAL ANTHROPOLOGY.

Physical anthropology, which includes somatology and physiology, has received considerable attention at the hands of some of the European anthropologists. Naturally, these sciences are studied at immense disadvantage when confined to prehistoric man; therefore it has been extended to include savage peoples, and many of the most ardent anthropologists of Europe have studied the somatology and physiology of the savage in the endeavor to obtain even reflected light or knowledge concerning prehistoric man. There had been a number of skeletons of prehistoric man found throughout western Europe. The instances are rare and isolated where specimens have been found of Paleolithic man. The evidence has not always been harmonious, nor has it always pointed in one direction. The Neanderthal skull has been assumed as the representative of the oldest race. Probably a dozen other specimens

of human skeletons, or fragments thereof, have been found, all of which are claimed to have belonged to Paleolithic man. The following are the best known : Canstadt, 1700 ; Lahr, 1823 ; Engis, 1833 ; Denise, 1844 ; Neanderthal, 1856 ; Olmo, 1863 ; Naulette, Furfooz, Solutré, Cro-Magnon, Engisheim, Savona, 1865 ; Aurignac, Laugerie, Brux, 1872 ; Mentone, 1872-75; Spy, 1886. Those of the Grotte de Spy, in Belgium, are the best identified and authenticated.

The conclusions to be ventured are, that Paleolithic man had a dolichocephalic skull with prominent frontal sinuses ; he was short in stature but had heavy bones with strong muscular attachments. He was prognathous, with large and strong projecting teeth which were unusually sound. He had habitually, three molar teeth. His legs were crooked, and it has been doubted whether he regularly assumed an upright position.

The human remains found in the caverns, still Paleolithic but of the later epochs, indicate an increase in height, size, and symmetry. It has been supposed, from comparison of osteologic evidence from the caverns, notably with the Cro-Magnon skeleton, that the Berbers, of North Africa, and the Guanches, of the Canary Islands, represent a similar ethnic type.

The Neolithic man had a skull more brachycephalic. He was not so prognathous as was Paleolithic man ; his forehead was higher and squarer, and his brain capacity greater ; his teeth were less projecting and not so large as those of Paleolithic man. The conditions of human burials in prehistoric times were not advantageous for the present study of the somatology of the individual. The Paleolithic man rarely, if ever, buried his dead, and when he did the preservation and discovery of the skeletons have been largely accidental. The Neolithic man buried his dead in great ossuaries and frequently, if not always, subjected the individual to a second burial after the integuments had disappeared. The immediate and direct result is that modern discoveries of these ossuaries find the bones pellmell, and we are unable to identify those of individuals.

CLASSIFICATION OF RACES.

Unable to obtain sufficient specimens to enable them to master the science in its relation to prehistoric man, the students of somatology have, as already suggested, extended their investigations to modern peoples, primitive and savage, hoping for two results : one, incidentally a knowledge of these peoples *per se*, and the other to obtain by comparison a better knowledge of prehistoric peoples. This investigation induced classification of races which have run into infinitesimal details.

There has been much striving and disputation among anthropologists for a satisfactory classification of the human race. The item in this classification which seems to have been received with most favor is determined by the cephalic index. This is the ratio between the extreme length of the skull as compared with the extreme breadth, and this as compared with the extreme height. Various subdivisions have been made and various names given : dolychocephalic, the long-headed ; mesocephalic, medium- and brachycephalic, short-headed. Other schemes are according to the character of the hair, running through lophocomi (tufted), eriocomi (fleecy), euplocomi (curly), and euthycomi (straight). Still another classification was that of the dental index by Prof. W. H. Flower : the microdont (the lowest index), mesodont (medium), and megadont (the highest dental index).

The earliest and possibly original scheme of classification of the human races was according to color : the yellow, white, black, to which were afterwards added the brown and the red. Probably these stand the test of experience in science about as well as the more complicated classifications.

Dr. Topinard has undertaken an investigation among the people of France by which he is to determine the color of the hair and eyes, segregated according to different departments. Virchow has done the same among the school-children of Germany, and in a late work Dr. W. Z. Ripley, of Columbia College, New York, has reported and published sundry investigations in some of the countries of western Europe, classifying and separating the peoples according to color of skin, hair and eyes, of the cephalic index, of height, and other physical characteristics. Such work as that of Dr. Ripley applied to

the native races of America would be new and original, and a more valuable contribution to the science of anthropology can scarcely be imagined. Dr. Washington Matthews, U.S.A., made such an investigation of the early occupants of the Salado Valley, Arizona.

Darwin's discovery of the origin of the human species by evolution from lower forms of animals, created an interest in the antiquity of man different from that of archaeology. It required a knowledge of zoology and of human and comparative anatomy and involved a study of anthropology in its subdivisions of somatology, physiology, and psychology, involving the physical and intellectual characteristics of man. Based upon this necessity, various schools and societies of anthropology were organized in many of the great cities of the world, notably Paris, London, and Berlin. The organization of these societies and the investigations involved brought to the front a set of scientists totally different from those who had before been studying archaeology.

Broca, in Paris, stood near the head of these, followed by Manouvrier and Topinard; Gosse in Geneva, Huxley and Tylor, Beddoe and Keane in England, Virchow, Bastian and Meyer in Germany, with Mantegazza and Sergi in Italy. The family Bertillon, consisting of the father (now dead) and his two sons (successors) were the discoverers and inventors of the science of anthropometry in its adaptation to prehistoric man. The races of men had been studied before, and the general divisions were those by color. Anthropometry gave an additional interest to this branch of the science and it ran riot, making subdivisions on the bases of infinitesimal details. This was pressed to such a point that one ardent investigator found sufficient difference in the human species as that he subdivided it into 172 races.

ANTHROPOLOGY, THE SCIENCE OF MAN.

Anthropology was defined to be the science of man, and included everything related to man, his physical, intellectual, psychologic characteristics; and these extended through all ramifications.

SUBDIVISIONS OF ANTHROPOLOGY.

Some scientists, chiefly the French, have proposed to confine the term "Anthropology" to the physical structure, but it is deemed better to include within it everything pertaining to man, making the various subdivisions as represented by the minor sciences, even though they might be treated independently. The following is little more than suggestive:

Biology and comparative anatomy :
 Human anatomy,
 Anthropometry, craniometry.
Comparative psychology:
 Literature, language (written, oral, sign),
 Religious creeds and cults.
Industry :
 Materials and implements of every craft,
 Clothing and personal adornment,
 Habitations and household utensils,
 Weapons,
 Objects for amusement,
 Articles, uses unknown.
Architecture. Fine arts:
 Monuments and public works,
 Roads, trails, canals, irrigating, etc.,
 Mounds—sepulchral, effigy, altar,
 Forts and earthworks,
 Graves and cemeteries,
 Idols and temples,
 Cliff or cave dwellings,
 Towers, ruined or otherwise,
 Engraving,
 Painting,
 Sculpture,
 Ceramics,
 Decoration,
 Ornamentation.
Sociology :
 Love, marriage, child-life,
 Social organizations, customs, and belief,
 Pastimes, games,
 Tribal organization,
 Government, property, law, etc.,
 Mythology. Folklore,
 Education, relief, and charities,
 Mortuary customs and grave furniture.

The subdivisions made by the Society of Anthropology of Paris, as set forth in the course of lectures given by its professors during the present year, are as follows :

> Prehistoric Anthropology,
> Anthropometry and Embryology,
> Ethnology,
> Biology,
> Language and Ethnography,
> Sociology (history of civilization),
> Zoologic Anthropology,
> Geographic Anthropology,
> Physical Anthropology.

The Society might not accept the foregoing as a correct or complete subdivision of the science. Other branches may be added on the employment of more professors.

The Society of Anthropology at Washington has, during the past year, made the following rearrangement of sections according to what was deemed proper in matter and terminology :

> Section A. Somatology,
> "   B. Psychology,
> "   C. Esthetology,
> "   D. Technology,
> "   E. Sociology,
> "   F. Philology,
> "   G. Sophiology.

It will be understood from the foregoing that the subdivisions cannot be made on hard and fast lines, but are susceptible of infinite changes and varieties. It would be scarcely possible for any one to master all these sciences and so become a perfected and all-round anthropologist. Classification, however, is largely a matter of definition ; the material facts remain the same. The field of any particular science is well understood, whatever name may be given or to whatever classification it may belong, and it is not worth while to engage in extensive discussion of any particular classification or the nomenclature or terminology of any of these sciences. It is deemed more satisfactory to group them all under the generic name of "Anthropology." This plan has been pursued generally in the societies of Anthropology and in the educational organizations where it is pretended to be taught.

UNITED STATES.

It is my duty on this occasion to give some expression to this subject in its relation to America or to the Western Hemisphere. The length of this address precludes an exhaustive examination. The student or reader interested might, before proceeding further, read the addresses delivered before this Section, the first by Dr. Daniel G. Brinton[1] at New York in 1887, the title being "A Review of the Data for the Study of the Prehistoric Chronology of America;" and the second that of Dr. C. C. Abbott, at Cleveland, in 1888, the title being "Evidences of the Antiquity of Man in Eastern North America."

The conditions under which the beginnings of our knowledge of prehistoric man were made, were quite different in America from those of Europe. In Western Europe the historic period began with the invasion of Caesar, fifty or more years before the Christian era, and the prehistoric period with which we have had to deal came to a close about that time.

On the contrary, in America the prehistoric period continued until the discovery of the country by Columbus and its subsequent occupation by the white man who was thus brought face to face with the prehistoric man. The superstitions, myths, and folk-lore concerning stone hatchets and flint arrow-heads so prevalent in western Europe, had no place in America. It was useless to talk to the white man of the heavenly origin of the stone hatchet or the flint arrow-head, when he knew by the evidence of his own senses that these were the implements and weapons of the prehistoric savage with which he had to deal.

PALEOLITHIC AGE IN THE UNITED STATES.

The existence of the Paleolithic stage of culture in America has been doubted, and indeed strenuously denied by some of our scientists who are well up in archaeology and prehistoric anthropology.

My somewhat extensive travels with long stops and continuous examinations of many of the localities in Europe occupied by Paleolithic man especially among the caverns of Dordogne district, my personal acquaintance with most of the collections

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[1] Died at Atlantic City, July 30, 1899. Resolutions of condolence were adopted by Section H at the next meeting after the delivery of this address.

of Paleolithic implements made in these countries, my association with the leading investigators and believers in Paleolithic
occupation, have fitted me in a degree to judge of the subject
which it would be mock modesty on my part to deny ; while
my dozen years' service in the prehistoric department of the
U.S. National Museum gives me an acquaintance with the American specimens by which I may compare the specimens from the
two countries in a peculiar manner, which I hope is not without its value.

The original discovery of a Paleolithic period was made in
Europe. The determining characteristics of that period have
been decided only in Europe, and it must be principally by
comparison with the evidence there that we are to determine
the existence of a corresponding period in America. This evidence is furnished (in Europe) largely by geology and by
palaeontology. As has been described, discoveries of the remains of man, either physical or (industrial?) technologic,
have been made in, and belong to, quaternary deposits, determined either by the geologic strata in which they were found
or the palaeontologic objects with which they were associated.
This species of evidence is, to a considerable extent, lacking
in America. The European conditions have been found to
exist in but few localities, but America is not entirely without
instances. Dr. Koch found a mammoth skeleton in Missouri,
associated with which were flint weapons of human manufacture. It and the weapons are now displayed in the Berlin Museum. Dr. Dickeson found at Natchez, Mississippi, the buried
skeletal remains of a Megalonyx superposed on a portion of a
human skeleton. The human skeleton from Guadeloupe, now
at Paris, was encased in coquina, a rock made of shells belonging to the quaternary, though not exclusively so. The Iron
Man of Sarasota Bay, Florida, found by Judge John G. Webb,
was completely fossilized and changed to limonite. A fossilized human calcaneum was found by Col. Joseph Wilcox, of
Philadelphia, in the same neighborhood with a quaternary
shell forming part of the mass. Three similar instances were
found in the same county in separated localities, showing them
to have been different individuals ; some of these have been
encased in bog iron ore, others in indurated sandstone appar

ently as solid as though formed at the bottom of the ocean. The Nampa Image has been cited as evidence of high antiquity of man in America, and while its genuineness has been questioned, the attacks upon it are far from being successful.

The Calaveras skull has been the subject of much hilarious scientific criticism bordering on contempt. The facts of its discovery should be subjected to painstaking and detailed investigation before the results of those facts are assumed. Whatever may be the conclusions concerning the fraudulent character of this specimen based upon its alleged "planting" by contemporary miners, as a practical joke to "fool Professor Whitney," it should be remarked that the evidence favoring this charge is itself open to as grave suspicions as is the rankest fraud ever perpetrated. The geologic changes of that country have been so great that it requires the gravest consideration and an intimate study or knowledge of all the facts before any one is justified in passing upon the archaeologic question. I cannot here or now investigate the subject from either of these view points. I am not a geologist and I have never visited the locality. I can only suggest some of the points to be considered before a conclusion is reached, and raise a warning or danger flag to those who would decide against the authenticity of the specimen on insufficient or *a priori* grounds.

The Stanislaus river, at the time of the deposition of the lava and gravel in which the skull was found, ran down the side of Table mountain in the same neighborhood in which it now runs ; but its valley was then some fifteen hundred feet higher than at present ; that is, since the valley of the Stanislaus was choked up and the water turned aside by the eruption of lava and the deposit of cemented gravel, the deflected river has cut or eroded a new channel fifteen hundred or more feet deeper into the earth than was the earlier channel. This will give some idea of the immensity of time and the great surface changes with which we have to deal. Many implements and objects of undoubted human origin have been found in divers' localities in California, alleged to have been imbedded in the same kind of gravels and to have formed part of similar deposits, and it is part of the argument against the Calaveras skull to

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assail the authenticity of their discovery. First it was charged that these finds were made by miners, laymen, ignorant and unaccustomed to recognize or describe them with scientific accuracy ; but this was answered when Professor Clarence King, then head of the Geological Survey of the United States, and the highest scientific authority, found one of the pestles *in situ*, imbedded in the cemented gravel under the lava cap, that he recognized its character before he exhumed it and in view of the importance of the questions involved, proceeded with care to dig it out. He preserved it, brought it to Washington, and placed it in its lawful depository, the U. S. National Museum, where it now is. It is remarkable that similar implements and objects to the number of several hundred should have been found, alleged by their finders to have been dug out of the gravels under the lava cap in various localities in California— it is remarkable, I say, that these should all have been frauds, and their finders either swindlers and liars, or else have duped themselves by their own discoveries. California miners have been generally credited with more astuteness than to be their own dupes, while it is curious if a whole state or a whole class within a state should combine in a general swindle and lie, out of which no profit, present or prospective, was possible. The objection has been made that these implements are polished or ground, at least pecked or hammered ready for polishing, therefore belong to the Neolithic or polished stone age ; and this it is alleged is incompatible with their great antiquity. Some American archaeologists assert that chipped stone implements were more difficult to make than polished ones, and on the well-recognized principle that the simplest and easiest way was earliest, while the more complex and difficult ways came later, they insist with pertinacity that European classification is erroneous and that the relative chronological positions of the Paleolithic and Neolithic ages should be reversed. This view, if accepted, would satisfactorily explain the apparent anomaly of the California implements. The real answer to this objection is that we know but little concerning California prehistoric archaeology. It presents many problems which have not been solved, nor indeed do we seem to be in the way of solving them. Some of these are as follows : The Indian languages of

the Pacific slope have peculiarities as yet unexplained. A fringe of country lying between the coast range and the ocean contains a greater number of stocks or families of languages (29), than all the rest of North America combined.[1]

The reason for this has never been explained even theoretically or tentatively. The arrow-points and spear-heads of the Pacific Coast are notably different from those of other parts of the country. To such extent is this true that in my classification of these implements and weapons,[2] I was compelled to make a separate class for the accommodation of the implements from this district. Pottery forming the most serviceable, and which might be considered the most important, domestic utensils, and as such used by nearly all prehistoric and primitive peoples, makes complete default on the Pacific coast ; this too while their neighbors the natives of Mexico and the Pueblo country, even the wild and savage Papagos, make and use it continually, some being of the largest forms with the finest decorations. Basketry in some cases supersedes pottery for carrying liquids, and the finest in America and perhaps in the world, either in ancient or modern times, are to be found on the Californian coast.[3]

The ollas (carrying or cooking jars taking the place of pottery), are made of serpentine instead of clay. These are some of the California anomalies. When the problems presented by them have been satisfactorily solved, that relating to polished stone implements may not appear so difficult.

It has been objected that the stone implements of seemingly so high antiquity were not water-worn and bore no traces of long or distant transportation by the mountain streams. An answer is patent. There is no evidence that they were transported or rolled any distance by water, and until this fact be established, there is no need to attempt the demonstration of its cause. We should establish the fact before we explain its cause.

The study of California archaeology, in order that it be satisfactory, requires a union of three scientists, the archaeologist,

[1] See Major Powell's linguistic Map, Seventh Annual Report of the Bureau of American Ethnology, 1885-86, pp. 7-142.

[2] Report of the U. S. National Museum, 1897, pp. 811-988.

[3] See the Hudson collection just purchased and now in the U. S. National Museum.

the geologist, and the historian, who shall act as lawyer or judge. The Calaveras skull incident has closed, has passed into history, and its facts are to be determined by evidence, the same as any other fact in history. The first question is, did Mattison actually find the skull as he says he did? and second, had it been planted in order to "fool Professor Whitney?" I think if this issue was made up to be tried before a court and jury on the lawful evidence submitted, the answers would be in the affirmative on the first question and negative as to the second. Until this issue is determined, it is folly to try the case by popular clamor and to denounce its possible believers or pour vials of contempt and contumely upon their heads.

Because I have favored the authenticity and genuineness of specimens which have been assailed, I would not have it understood that I am deluded into the belief that all specimens are genuine. I recognize that frauds have been committed, that fraudulent specimens have been manufactured, planted, dug up and sold as genuine, and that great deceptions have been practiced. I have not hesitated to attack and destroy their claims whenever presented ; but I here contend that in passing on the genuineness of specimens, we should decide fairly and honestly. We should first get possession of all the facts, sift them to their last residuum of truth, and, giving each fact its fair and due weight, decide the question according to our best and truest judgment. This should be done "without prejudice or preconceived opinion." It is unfair to decide such questions in advance of knowledge of the evidence ; it is unscientific to decide *a priori* that so-and-so is true because it must be true, and so-and-so is not true because it can't be true. In determining these contested questions, I have ever sought to be impartial and, above all things, honest. It is only thus that we can hope to arrive at the truth.

Boucher de Perthes' discovery of Paleolithic implements in original or undisturbed quaternary river gravels has been described in its appropriate place in this address.

After the proposition that these were remains of human industry had been accepted, the European investigators drew deductions based on the similarity of objects and implements found in other localities where the geologic or palaeontologic

evidence was not so plain or so plentiful, and the finding of Paleolithic implements alone has been accepted as evidence of human occupation during that period. The same practice has been pursued in America. The deposit at Trenton, New Jersey, is accepted by geologists as belonging to the quaternary period; and while the Paleolithic implements or the human remains therein found have been disputed, it seems to have occurred so often and these finds have been so numerous that it cannot long continue to be denied. The discovery of a mammoth tusk in the Trenton gravels, now on exhibition at Rutgers College, New Brunswick, N. J., is confirmatory evidence not to be overlooked or lightly regarded. I do not propose to enter into a discussion of the weather-beaten subject of the Trenton gravels. I presented a paper before this Section at the Detroit meeting,[1] by which I still stand. The same sort of evidence is furnished by the Tuscarawas specimen found by Mr. Mills in the glacial till of Ohio, and described by Professor Wright;[2] likewise the implement found by Dr. Hilbourne T. Cresson, Delaware, and made the subject of a paper by Professor Wright, read before this Section at this meeting.

The chapter on High Plateau Paleoliths, deals with paleolithic chipped flint implements found in England on the surface; others of the same nature have been found, still on the surface, in France on the high plateaux between the rivers Seine and Yonne. These have been recognized by every one who is competent to have an opinion, as true Paleoliths. The same condition applies to certain localities in the United States, that is to say, on the plateaux at the head-waters of certain rivers beyond the erosion by which the valleys were formed. So there have been found on the surface in the United States many chipped flint implements which from their size, shape, appearance, and mode of manufacture, are identical to the smallest detail with the recognized Paleolithic implements of Europe. These are dissimilar to the prehistoric implements of every other period in any country, and if there is any force or truth in the argument of similarity of culture from, or by reason of similarity of implements, between two widely separated peo-

ples using them, this would seem to establish the existence of a Paleolithic period in America as well as in Europe. Dr. Brinton and Professor Putnam, though occupying antagonistic positions on many of these questions, both seem to concede the antiquity of man on the American continent.

Dr. Brinton's address, heretofore mentioned, contains two or three pregnant sentences on the subject of man's antiquity in America which, coming from him, are noticeable, and I quote them approvingly :

There is, however, a class of monuments of much greater antiquity. * * * These are the artificial shell heaps which are found along the shores of both oceans and many rivers in both North and South America. They correspond to the kitchen middens of European archaeology. * * * The shells are by no means all of modern type. Many are of species now wholly extinct, or extinct in the locality. This fact alone carries us back to an antiquity which must be numbered by many thousands of years before our era. * * * This class of monuments, therefore, supply us data which prove man's existence in America in what some call the diluvial, others the quaternary, and others again the pleistocene epoch, that characterized by the presence of extinct species.

Professor Putnam, in his address at this meeting, said :

"That man was on the American continent in quaternary times, and possibly still earlier, seems to me as certain as that he was on the Old World during the same period."

ANTIQUITY OF THE RED RACE IN AMERICA.

Not to split hairs over names, I suppose we would all agree upon the generic name of "Red Race," and as I have some definite opinions as to the antiquity of the Red Race in America, I may make a résumé of my position.

If we accept the theory of the unity of the human species and its origin from a single stock, we must agree that the human species either originated on the western Hemisphere and migrated to the Eastern, or else the reverse. Whether it originated in America or came here by migration, the conclusion seems irrefutable that it started with but comparatively few individuals, they occupied but one or few localities, they grew to have practically the same industries, and they spoke practically the same language. Prof. Putnam[1] contends that there was more than one race and so there may have been more

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[1] See his Presidential Address.

than one migration and more than one colony. This, if accepted (and I make no dispute over it), does not materially affect my proposition. There were surely but few colonies, with but few members in each. From these small beginnings, the red race had, prior to the discovery of America, spread over the entire hemisphere, from the Arctic Ocean to Terra del Fuego and from the Atlantic to the Pacific ; it had increased, we can only suppose, in the natural way, from a single pair or score or possibly a hundred individuals, to the seven or eleven millions which are said to have been the numbers at the time of Columbus' discovery ; and their migrations had been sufficiently extended and the separation sufficiently pronounced and maintained, as that the language originally spoken had increased to the great number of which we now know.[1]

There is a difference, or distinction in the ground or polished implements and objects of the ancient man of North America, which indicates a high antiquity. The Indian made and used, at the time of the discovery, certain implements and objects which have been continued in modern times, by which he can easily be recognized and identified. Many of these are of the same type as those in Europe in Neolithic times. But there are certain others, also ground, polished, and drilled, some showing a high order of mechanism, art, and industry, which had gone out of use and had become prehistoric among the Indians themselves. They have been found in mounds and show a pre-Columbian and ancient origin. The objects referred to are usually, of the class termed ceremonial : banner-stones, bird-shaped, boat-shaped, spade-shaped, gorgets, tablets drilled or inscribed, sinkers, pendants or charms, tubes, and certain specimens of stone pipes. The mounds themselves indicate a great antiquity, but their building and use seems also to have continued into later and possibly into modern times. The antiquity of the mounds has been a subject of great contention, but I refer to a foregoing quotation from Dr. Brinton, and also to the address of Professor Putnam[2] delivered at this meeting, where he says :

[1] The Bureau of American Ethnology estimates the number of the different stocks among the American Indians at fifty-six ; while the number of languages is estimated at two hundred and over.

[2] Ante p. 342.

Many of these shell mounds are of great antiquity * * * and cannot be regarded as the work of one people. * * * Thus it will be seen that the earth mounds, like the shell mounds, were made by many people and at various times. * * * So far as the older earthworks, such as Newark, Liberty, Highbank and Marietta group, the Turner, the Hopewell group, the Cahokia mound of St. Louis, the Serpent Mound of Adams County, Forts Ancient and Hill, and many others, have been investigated, they have proved to be of very considerable antiquity. This is shown by the formation of a foot or more of vegetable growth upon their steep sides, by the primeval character of the forest growth upon them, and by the probability that many of these works, covering hundreds of acres, were planned and built upon the river terraces before the growth of the virgin forest.

If the above facts in regard to the origin of man on the Western Hemisphere be accepted as true (and it is difficult to see how they can be evaded) the conclusions announced of the high antiquity of man in America seems to be incontrovertible; and I am glad to stand with Dr. Brinton and Professor Putnam in maintaining the same conclusion, however much we may differ as to the arguments by which it is reached.

We have assumed a migration from the Eastern Hemisphere as a means of accounting for the human occupation of the Western; how it comes that the human product in the Western Hemisphere should be different from its progenitors in the Eastern, is not involved in this discussion. That question belongs to the earlier one of the origin of races. If we question how the red race of America could have sprung from either one of the three or four races of the Eastern Hemisphere, we are involved in equal obscurity as to how the three races of the Eastern Hemisphere should have sprung from a single stock, assuming, as we have, the unity of the human species. The discussion of this question is not here pertinent; it belongs to another branch of the science of anthropology and is to be discussed elsewhere. If we accept the theory of the unity of the human species and that they all sprung from one stock, the conclusion may as well be accepted as to the formation of the red race in America, as the yellow in Asia, the white in Europe, and the black race in Africa. The problem of the peopling of America has been dealt with theoretically by M. de Quatrefages in his "Histoire Generale des Races Humaines," wherein he assumes a combination of thirty individuals of the

yellow, twenty of the white, and ten of the black race, who, placed on the common basis of an isolated colony anywhere in the Western Hemisphere would, by amalgamation and procreation, produce a race with the principal characteristics of the red.

MIGRATIONS OF RED RACE IN AMERICA

Continuing our stand on the theory of the unity of the human species, we recognize that all the different races must have sprung from one stock, and this could have been done only by the most intimate physical connection. No theory of similarity of human thought and need will even assist in explaining this fact. The difficulties of migration all disappear before it; distances of time and place are as nothing. On the basis that the human species sprang from a single stock, the conclusion is not to be evaded that all the races, the red among the rest, descended from that stock, generation after generation, from father and mother to son and daughter; and this must have been true from the time of the first human pair down to those born in A.D. 1899. This proves the communication and relationship between all individuals of the human species, and *a priori*, that all human occupation of different countries, or passages from one country to another must have been accomplished by migration.

On this subject Sir John Lubbock ("Prehistoric Times," p. 587) says:

Assuming, of course, the unity of the human race, there can be no doubt that man originally crept over the earth's surface, little by little, year by year, just, for instance, as the weeds of Europe are now gradually, but surely, creeping over the surface of Australia.

On this assumption, the question of human migration, and with it the migration or importation of human industries settle themselves. If the people migrated, they carried their industries with them. Their knowledge of implements, utensils, and weapons, and how to make them, ought to be substantially the same in both countries, the country of immigration as in the country of emigration, and this we find to be true.

If the prehistoric man migrated from the Eastern Hemisphere to the Western, and commenced his occupation at the early

period, the Paleolithic, as suggested by Dr. Brinton, and as indicated by the existence here of possiblePaleolithic implements, he must have brought with him the knowledge of Paleolithic industries, whatever those may have been. He may have come over in the Paleolithic period and had either a continued communication or a renewal of the migration. If his migration or the renewal thereof was not until the Neolithic period, then he brought with him a knowledge of that period. If we are to determine this by the similarity of industries, we would say that the last migration in prehistoric times was during the Neolithic period. Waiving for the moment any discussion as to whether the man of the Neolithic period was still in the savage stage of culture or had advanced to the barbaric, it is remarkable that the industries between the two countries should have been so nearly identical. Nearly every industry that would belong to a savage or barbaric people which might be regarded as necessary to their comfort, if not their existence, is found in both hemispheres, and in both substantially alike. In many industries, that is in the making and use of many implements, utensils, or weapons, they were exactly alike. There was, in these cases, an absolute identity; the differences were not greater between the implements, etc., of the two hemispheres than between those of any two countries in the same hemisphere.

SIMILARITY OF HUMAN CULTURE NO EVIDENCE OF SIMILARITY OF RACE, BUT IS OF INTERCOMMUNICATION.

The similarity between man's culture in Europe during the Neolithic period, and that in America during the pre-Columbian period, extended to nearly every industrial object of importnce relating to the lives of the two peoples. Nearly everything relating to tools or implements which one generation or one people could teach another existed in both countries. I speak, not of the tastes, habits, customs, folk-lore, games, traditions, religious beliefs, etc., which may or may not have been continued from one country to another,—these may have been lost in transmission,—but of the serious things of life, those which go to make epochs of culture, which determine the civilization, questions involving sustenance of life, such as imple-

ments, utensils, weapons, the means by which life was maintained and made possible. I may speak, also, of the tools with which these implements were made and the method of their manufacture.[1]

The lines on which this parallel is drawn are so broad as to include practically all savage or barbarian needs. The industries of chipping, battering, pecking, grinding, polishing, sawing, and drilling were all applied to stone, bone, horn, and wood, and were identical in Europe and America. The implements made from these materials and by these methods were similar, if not identical, in the two countries: stone hatchets, bow and arrows, spear-heads, knives, scrapers, grinders, mortars and pestles, gouges, chisels, hammers. There is not more difference between these tools in the two hemispheres than there is between them in any two countries in the same hemisphere. A series of polished stone hatchets from Scioto Valley, Ohio, will, save only the difference in material, correspond favorably in form, size, mode of manufacture and possible use with a like prehistoric series from almost any country in the world. The same is true of all the implements mentioned in the list above. Pottery, which figures so extensively in the life of primitive man, was substantially the same in the two hemispheres; spindle-whorls and thread, on which depended the art of weaving and all the paraphernalia of nets and snares for catching game; these, like the others, were practically the same in both hemispheres. There were differences in size, weight, material, and in ornamentation of spindle-whorls, but throughout the prehistoric period they were substantially the same utensil.

We find plenty of prehistoric weaving, more in Europe than in America, probably because the latter peoples used skins for clothing and tents; but the invention and use of the loom by which the manufactured product of the spindle-whorl could be utilized, was a machine of great intricacy and difficulty of manufacture. This intricacy and difficulty became magnified when we consider that the loom and the spindle-whorl are but parts of the same machine and that to a large extent each de-

[1] The architecture and possibly the sociology of the Aztecs, in Mexico, and the Incas, in Peru, should be excepted from this general statement and be subjected to special investigation.

pended on the other. When we find the machines and their products practically the same in both countries, it is an argument of great weight in favor of contact or communication between the peoples and carries with it a power of conviction.

One of the intricate and important industries in primitive life, whether savage or barbarian, was the treatment of skins of animals for tents or clothing. The first and most necessary implement for the treatment of skins is the scraper, and this is as true of the modern tannery as it was in the time of the shepherds on the Plains of Chaldea. The prehistoric scrapers of Europe and America are identical. The skins of prehistoric times in both countries, whether tents or clothing, have perished, and no traces of them are found; but the flint scrapers with which they were treated remain, and are now found a satisfactory and convincing evidence of the treatment of the material, and that in both these the early men of Europe and America were alike.

Lest some critic should pick a flaw in the foregoing statement of facts, I mention the teshoa, a kind of scraper peculiar to the foot-hills on the eastern slopes of the Rocky Mountains. It was described by Professor Leidy, and specimens have been sent to the Museum by Col. P. H. Ray. They were simply spauls from boulders with a sharp edge and were knocked off by the Indians during their buffalo hunts, used temporarily, and left. This is believed to be the only exception to the universality of the form of the stone scraper in the Neolithic age throughout the world.

Speaking of the similarities between the industries and implements of the two hemispheres, I have used the term ''identical,'' and the word is correct. There may be a difference in detail, arising from the separation of time and distance, but with all that, they were the same industries, the implements were the same, made of the same kind of material, by the same process and to serve the same purpose. If there is a difference between these industries and objects in the two hemispheres, it is like the difference between the present fashions in dress in France and in the United States. But there will be a difference between the fashions of Paris and London, or, to make it more patent, between the city-folk and

the peasants, whether of France, Holland, Sweden, Scotland, or Ireland. So are there differences between the fashions of the various cities or states in the United States; yet in all these countries, among all these peoples, however widely separated they may be, the difference is only of fashion; and all the costumes worn are at last the same articles of dress. This is fair illustration of the differences between the stone hatchets or the arrow-points and spear-heads of prehistoric times in the countries named.

In Europe the stone hatchet was inserted in its handle, though there may have been variations of the mode of fastening. Arrived in America, we find the same stone hatchet, handled also by insertion. When the European Neolithic man wanted an axe or heavier chopping or splitting implement, he drilled a hole through the axe and inserted a handle sledge-fashion. The prehistoric American did not adopt this style. He made a groove and tied a withe around his axe. This was a difference in detail between the style of implement of the two countries. It was not because the European man did not know how to make a groove and put a withe around it, for his mining tools were made in that way; nor, on the other hand, was it because the American could not drill a hole in stone, for he drilled as much and as finely as did the European.

There were other differences of detail. The pottery of America may be larger and more finely made, but in both hemispheres the processes were practically the same. There is as much difference to-day in pottery-making establishments in adjoining shires or counties in either of the two countries, as there was between the countries themselves. Ornaments of stone and shell may be different in the two countries, but they are at last but ornaments, and as such have their local fashion.

There may be other differences with other implements and industries, but they are of degree rather than of kind. I think I may fairly stand on the proposition that there will be found as great differences between the primitive or prehistoric industries in each country, for example between those of the Atlantic and Pacific Coasts of America, between those of the United States and Mexico and Central America, as will be found between those of Europe and America; so also will there

be as much difference between the industries and implements of the dolmen people and the lake dwellers, or between those in the Scandinavian and the Iberian Peninsulas.

In the foregoing differences between the two countries, the subject of basketry may serve as a good illustration. We have just received, at the United States National Museum, a fine collection of primitive or aboriginal basketry from California, representative of the Pacific Slope. It differs greatly from the prehistoric basketry of either Europe or the Atlantic Slope in that it is much finer and better made, but the stitches and plaiting are on the same general system and done in the same general style. While the difference is marked it is at last one of detail. The theory by which this may be explained is that the art has become perfected in California, not alone since the migrations from Europe, but since the establishment of the Indians on the Pacific Coast.

Bronze found no lodgment in North America. A good explanation is that the migration from Europe, by which America was peopled, took place prior to the advent of the bronze there. There might have been more than one migration to America; one during the Paleolithic, and a later one during the Neolithic period; but it seems not to have been repeated after bronze became known in that country.

The principles which underlie this argument of similarity of industries as proving migration or communication or contact, do not depend alone upon the similarity of the objects, but also upon the difficulty of manufacture and performance, the intricacy of the operation required, the skill of the workman; and to these may be added the closeness of resemblance, the similarity of detail, and the number of repetitions. A single specimen, or a few specimens having only an insignificant or uncertain similarity, might be of no avail in establishing the proposition of migration or communication of peoples between the countries; while, as the resemblances are increased, and an increase in the intricacies of manufacture, in the difficulties of performance, in the skill required to make or operate the tool or machine, would very materially increase the testimony in favor of migration, and add weight to the evidence. This increase in weight is not in proportion to the increase in

number of examples, but rather as the square of that number.

The theory that the similarities of human thought account for the similarities of human culture in widely separated countries and among peoples without prior communication, savors of gross materialism, and is to be rejected as erroneous. That there are similarities of human thought is to be admitted, but if these control man in his progress and compel his passage in a materialistic or predestined path, they rob him of his free will and make him only a creature of circumstances. The best illustration I can suggest proving the error of this theory, is the action of human thought as manifested in human speech or writing. We may assume that human emotions, feelings, desires, and wishes are much the same among all people. Each human being loves, weeps, pities, hates, envies, etc., much the same as does every other. If they were to describe their feelings, one might expect to find it done in much the same language. Yet we know for a fact, that this is not so. If so done, it is charged as plagiarism. Of the thousands who have thus written, scarcely a "baker's dozen" have ever been thus charged. The reason most apparent is that with all the similarity of human emotions, feelings, desires, and wishes, the expressions thereof are so different when emanating from different authors that none lay themselves open to such a charge.

I am opposed to the theory advanced by certain anthropologists, that the similarity of human thought is a satisfactory explanation of the similarity of human culture in the case of widely separated peoples. That there is similarity of human thought between peoples, however widely separated, is conceded ; but I prefer to account for similarity of culture (especially industrial) among widely separated peoples by migration, or by communication or contact. If we accept the doctrine of unity of the human species, we are forced to admit contact between peoples of different countries as accounting for the differences in their cultures rather than to account for it by the similarity of their respective thoughts. The race could not have been perpetuated, the new peoples could not have been born, the different countries would never have been peopled, whether separated or not, except on the theory of migration

and communication or contact. It is only by contact that subsequent generations could have appeared, and only by migration that they could have become separated. If the spread of culture by migration is denied, the spread of the race must also be denied. The two things, similarity of race and of culture, stand on the same foundation. This foundation is migration, communication, contact.

MONUMENTS, BURIAL MOUNDS, AND TUMULI.

Nothing has yet been said as to the monuments or art of prehistoric man. The art is sufficiently explained in my work on "Prehistoric Art," published in the Report of the United States National Museum for 1896, pp. 325-664, with 74 plates and 325 text figures, and I need not dwell further thereon.

The monuments of prehistoric times are curious and strange. Whatever country we may consider, they excite our wonder and admiration. The ingenuity, invention, thought, and general *savoir faire* of the prehistoric man as shown in his industries ; and the taste and genius shown in his art pales before his ability as an architect and builder.

The principal monuments made by prehistoric man in most countries and times seems to have been funereal. The Paleolithic man made no monuments, and it is doubtful if he habitually buried his dead. But the Neolithic man expended his energies and powers in the erection of tombs and monuments intended to protect, and possibly to commemorate, his dead.

Dolmens are chambers of stone in which the dead bodies were placed. Mounds were frequently erected over such burials, and these stand as testimonials of the affectionate regard with which the barbarian of prehistoric times, whether in Europe or America, regarded his dead.

Although these monuments may not be the same in the details of their construction in both countries, they are all founded on the same principle of regard for the dead. This remark applies equally to Europe as to America. The burial tumuli and dolmens of Lozére and Morbihan in France do not contain a greater number of bodies than those of the Turner, or the Hopewell group, in Ohio, while for size, extent and compli-

cated design and perfection of execution, those we are to see during this session at Newark, Circleville and in the Scioto Valley will equal any throughout Europe.

The military monuments, fortresses, embankments, squares, circles and breastworks of the two countries tell the same story. They were built in both countries, sometimes of stone and again of earth, and show in every quarter an amount of engineering skill. The parallel lines at Marietta and Piketon, the circles and octagons on the State camp-ground at Newark, in the Scioto Valley, and at Portsmouth, Kentucky, have their counterparts in the extensive earthworks of protective ditch and embankment of Camp Peu-Richard at Saintes (Charente); while the fortresses and camps of stone or earth of forts Ancient or Hill, or opposite Bournemouth, are but the complements of Camp de la Malle (Alpes Maritimes) or the great Gaulish fortress of Uxellodenum on the Dordogne.

Other monuments in Europe occupy a relatively restricted area : menhirs, cromlechs, alignments, standing stones isolated or erected in circles, squares, or parallels belong to Western Europe but no corresponding monuments have been found in America.

I must conclude. My time and your patience are about exhausted. I recognize my shortcomings and apologize for them; but who can set forth within the limitations of a single address, the history of the first appearance of man on earth, describe a century's work in the discovery of a new science and complete at one stroke its classification and nomenclature ? The difficulty is increased when we consider that the want of harmony on these subjects is as great among our own scientists as it is between them and their foreign brethren.

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IN MEMORIAM.

Dr. Daniel G. Brinton died at Atlantic City, N. J., July 30, 1899.

The announcement of the death of Dr. Daniel G. Brinton is a shock to the Section of Anthropology, and brings sorrow to us, his fellow members. A zealous and critical investigator, Dr. Brinton advanced science with such efficiency as to rank among the foremost anthropologists of the world; a brilliant expositor of facts and principles, he diffused science with signal success; an upright man of humane motives and stainless life, his personal character brought prestige to science.

Dr. Brinton's interest in the advancement of science led him to early and active work in this Section and in the Association; and when he was distinguished first by the Vice Presidency, and later by the Presidency, as the highest honors in the gift of the organization, his ability and dignity, and the fame of his work, reflected and added honor on the Association.

Resolved, That we unite in an expression of grief for the death and of reverence for the memory of our former President, Dr. Daniel Garrison Brinton.

The preamble and resolutions were adopted and an engrossed copy ordered sent to the family, which has accordingly been done.

Obituary addresses were delivered by Profs. W J McGee and J. McK. Cattell. The military record of Dr. Brinton as surgeon and medical director in the United States Army during the civil war, was read by Vice President Wilson. Suitable remarks were made by Mrs. Herman and Professor A. W. Butler.

REPORT READ.

REPORT OF THE COMMITTEE FOR THE STUDY OF THE WHITE RACE IN AMERICA.

Your committee, as the Association and Science in America, has suffered a great loss in the death of its chairman. Ill health prevented Dr. Brinton during the last year of his life from forwarding to any considerable degree the objects of the committee in which he was so deeply interested. Our committee has only held one meeting, though there have been numerous consultations of two or more of its members, and we have individually done all in our power to further the study of the white race in America.

At the Boston meeting a grant of $50 was made to the committee for instruments to be constructed by Prof. J. McKeen Cattell. The money has been used for three anthropometric instruments, and the methods and results have been communicated to Section H in a special paper. Professors Cattell and Boas have used these and other methods to measure the physical and mental traits of the students of Columbia University. During the year the work has been extended to the women students of the University and to children in the Horace Mann School and certain pathological cases were studied.

Your committee has been in communication with the Committee of British Association on the Ethnological Survey of Canada in regard to the question of collecting anthropometric data in response to circulars of inquiry.

Your committee suggests the desirability of making certain physical and mental measurements of our own members at the annual meetings. This has been done for a series of years at the British Association, and a comparison of the measurements of scientific men in Great Britain and America would be of interest. We suggest that we be authorized to enter into communication with the Committee of the British Association with a view to suggesting to the similar associations of continental Europe the desirability of such measurements. We ask for a grant of $50 to arrange an anthropometric laboratory at the next meeting of the Association.

J. McKeen Cattell,
W J McGee,
Franz Boas,
William Wells Newell.

PAPERS READ.

[TITLES AND ABSTRACTS.]

THE SCIENTIFIC SOCIETIES AND INSTITUTIONS OF THE UNITED STATES. BY PROF. J. McKEEN CATTELL, Columbia University, New York, N. Y.

This paper, which was prepared as a monograph for the Government's exhibit at the Paris Exposition in 1900, was presented in abstract only. It gave an account of the origin, development, and present conditions of our scientific and learned societies, journals, museums, gardens, laboratories, and similar institutions for the advancement and diffusion of knowledge.

NEW ANTHROPOMETRIC METHODS. BY PROF. J. McKEEN CATTELL, Columbia University, New York, N. Y.

The instruments for these methods were secured by a grant to the Committee for the Study of the White Race in America, made at the Boston meeting to be expended by the writer. They are as follows : (*a*) A photographic method for measuring the features. Photographs were exhibited in which a portrait was taken with a centimeter netting close to the face. In this way the features both of the full face and the side face were automatically measured. A comparison was made between this method and ordinary measurements, and it was shown to have some advantage in accuracy as well as in convenience. The special value of the method, however, is that a permanent record is taken of an indefinitely large number of measurements, some of which may at the present time be regarded as unessential, but which might ultimately prove of value. (*b*) A method for measuring the quickness and accuracy of movement. Charts were shown which contained a hundred squares with sides of one centimeter and one hundred dots one centimeter apart. The observer is required in the first series to make marks in the squares as rapidly as possible, and in the second series to mark the dots as rapidly and as accurately as possible. Thus is obtained the maximum rapidity of movement and the relation of rapidity to accuracy. (*c*) A simple chronoscope. A grindstone, 2 centimeters in circumference, was adapted so that when turned once a second a light or sound suddenly occurred, while the reaction following it was registered with a pencil on the circumference of the stone. Owing to the momentum of the grindstone, it can be turned by

hand with a considerable degree of accuracy, and times varying by less than 1/100 of a second can be measured.

RESEARCHES IN EXPERIMENTAL PHONETICS. BY E. W. SCRIPTURE, Yale University, New Haven, Conn.

The results of an extended investigation of certain vowels and diphthongs were presented. The paper will be published in the *Studies from the Yale Psychological Laboratory*.

INADEQUACY OF THE PRESENT TESTS FOR COLOR-BLINDNESS. BY E. W. SCRIPTURE, Yale University, New Haven, Conn.

The usual test with the Holmgren worsteds is not adequate to excluding all the color-blind men from the railway and pilot service. The speaker related cases from his own experience. Similar experience has led the governments of Holland and England to add tests with colored glasses in front of a lantern. These tests are somewhat cumbersome and unsatisfactory. The speaker then exhibited and explained the principles of a new instrument, the color-sense-tester, designed specially to meet the case.

EVIDENCES OF PREHISTORIC MAN IN THE MAUMEE RIVER BASIN. BY DR. CHARLES E. SLOCUM, Defiance, Ohio.

The area of the Maumee river basin embraces about 8000 square miles, lying in Ohio, Indiana, and Michigan. Few evidences of prehistoric man have been found within this basin. Fulton county has twelve mounds, DeKalb eleven, and a circle 300 feet in diameter ; Steuben a few mounds and a circle; Paulding county seven; Defiance five ; seven in Allen county, Indiana; a few along the banks of the St. Joseph and the Auglaize rivers. They are of small size, thirty feet in diameter and four feet in height being the largest limits. The mounds are usually for burial purposes and contain from one to ten bodies; a gorget or two, a ceremonial object, a few arrow-points, and a vase or bowl are the extent of the accompanying articles. Later mounds have beads and wampum with an occasional silver charm (French). The early mounds are on the moraines, sandy beaches, or elevations outside the borders of the Maumee glacial lake, and were built before the lake receded. The later ones are not confined to this area.

CORRELATION OF THE GLACIAL DELTAS IN THE LOWER PART OF THE
    DELAWARE AND SUSQUEHANNA RIVERS. BY PROFESSOR G. FRED-
    ERICK WRIGHT, Oberlin, Ohio.

In 1887 Dr. Hilborne T. Cresson reported finding a palæolithic imple-
ment in a cut on the Baltimore and Ohio railroad in Claymont, Del., 19
miles south of Philadelphia. This cut is a mile and a half west of the
Delaware river, and 150 feet above it, and is identical in composition with
the Philadelphia brick clay and red gravel which have been correlated
with the Columbia deposits, so well exposed and developed in Washing-
ton. Near by the place of this discovery, and in deposits overlying the
horizon of the implement, were boulders several feet in diameter, which
must have been brought down the river on floating ice. The extreme
height to which these Columbia deposits have been traced in the Dela-
ware valley was estimated by Professor Lewis and myself, twenty years
ago, to be 180 feet. From this estimate Professor Salisbury did not ma-
terially differ, since he would not place the limit above 200 feet.

That the Columbia deposits are older than the deposits at Trenton, in
which Dr. Abbott and others have reported the discovery of palæolithic
implements, has always been recognized. But by some it has been
thought to be so extremely old, and to be separated from the Trenton
gravel by so vast an amount of physical change, and so great an interval
of time, that some who have accepted the genuineness of the discoveries at
Trenton have hesitated to give credence to Mr. Cresson's reported dis-
covery. In all my reports upon the subject, however, it has been main-
tained that the separation between the Columbia and the Trenton deposits
was not so great as supposed by many, and therefore would not raise any
insuperable objection to the credibility of Mr. Cresson's discovery.

This summer I have been able partially to complete my study of these
deposits by following down the Susquehanna river from Harrisburg to
the head of the Chesapeake bay, and examining the remnants of the old
delta of Columbian age still remaining about the head of the bay, and by
crossing over to the Delaware and revisiting Philadelphia and Trenton.
The results of the observations and comparisons tend to confirm the in-
terpretation given by Professor Lewis and myself. Briefly stated, the
facts, both new and old, are as follows : In the lower part of the Susque-
hanna river, there appear, in protected places, at an elevation of about
130 feet above the present low water-level of the river, gravel terraces
containing boulders two or three feet in diameter derived from the rocks
in the upper part of the valley. These are especially marked on the east
side of the river at Harrisburg, as described by Dr. Harvey Bashore.
(*Am. Jour. Sci.*, Feb., 1894, April, 1896). A similar terrace occurs at
Columbia, about twenty miles below Harrisburg. This was kindly
pointed out to me by Dr. Bashore. The elevation of the summit of the
terrace is from 130 to 135 feet above the river ; the material is gravel and
river silt, containing occasional boulders from two to three feet in diame-
ter. Remnants of this terrace were also found three miles below

Columbia, upon the east side of the river. Between this point and the mouth of the river, however, a distance of 35 or 40 miles, we were unable to find any remnants of this terrace, owing probably to the rugged character of the country, and the rapid gradient of the stream, which averages six feet to the mile. The principal remnants of the Columbia delta near the mouth of the Susquehanna are found on the islands and around the northern and eastern shores of the head of Chesapeake bay. These are specially prominent at Turkey Point, Grove Point, and Betterton, where Columbia deposits, containing numerous boulders from two to four feet in diameter, are clearly seen to cap the cliffs to an elevation of from 50 to 80 feet. The high-level Columbia deposits are not prominent, if indeed they exist at all, on the west side of the bay in the vicinity of Havre de Grace, and do not extend very far below Betterton. But they are found in the line of the projection of the Susquehanna valley on the line of the Chesapeake and Delaware canal half-way across from Chesapeake to Delaware, especially near Bucks, in the deep cut a mile or two east of the Maryland state line. In short, the relations of these deposits at the head of Chesapeake bay to the Susquehanna river are precisely those in the vicinity of Philadelphia to the Delaware river. In both cases they are delta deposits at the mouths of large streams the upper parts of whose drainage basins were, during the glacial period, deeply enveloped in ice, and during the closing portion of the period were gorged by enormous floods bearing large and numerous masses of floating ice. In both cases, too, it is clear that, at the time of the accumulation of these deposits, the land stood at a lower level than now. This, as we have seen, was estimated by Lewis at Philadelphia to mark a depression of 150 feet. The evidence about the head of Chesapeake bay is not quite so definite, but probably the depression was approximately the same. In neither case, however, was this depression enough to directly affect the formation of terraces as far up as Columbia on the Susquehanna or at Easton on the Delaware, though it is quite possible that the depression increased to the north so as materially to modify the gradient of these streams in the middle and upper portions of their course. Indeed, along the Susquehanna there is much evidence of such differential change in the gradient of the river bed; for, from above Harrisburg to the mouth of the Susquehanna river, the stream is running over a rocky bottom with a very rapid gradient, while above the glacial boundary, as pointed out by Professor I. C. White, it runs upon deep accumulations of gravel. This would seem to indicate that there had been in Quaternary times either a depression in the upper portion of the Susquehanna valley, or, more probably, a differential elevation across the lower part of the valley. If the river had been running for an indefinite period of time as at its present level and gradient below Harrisburg, it would have worn its bed nearer to a base level.

On the north side of the Susquehanna river, at its mouth, opposite Havre de Grace, there is an extensive terrace about thirty feet above tide-level extending for two or three miles which in a rough way corresponds closely with the terrace on the Delaware river upon which the city of

Trenton is built, though that is a little higher, being, in round numbers, fifty feet above tide level. But neither here nor at Trenton does there seem to be any necessity for an extremely long lapse of time between the higher level Columbia deposits and the lower terraces which on the Delaware river we had called the Trenton. On the other hand, the Columbia and the Trenton may well enough belong to a single continuous series of deposits during a period of rapid emergence of the land below the glacial border which took place during the closing stages of the glacial epoch. First, there is no hard-and-fast line of separation between the Columbia and what we call the Trenton deposits; and, on the Susquehanna, be_ tween those which we might correlate with the Trenton. Secondly, the Columbia deposits in both these valleys ,have suffered but slightly from erosion. This is a fact to which I have repeatedly called attention, and it is gratifying to have it indorsed by Professor Salisbury (see Report on New Jersey for 1895, p. 129), who arrives at the conclusion that the period of Columbia submergence must have been short from the fact that the flats of this material are undissected, '' even where in close association with considerable streams,'' which leads him to the conclusion that '' either the formation is very recent, or conditions since its development have been most unfavorable for erosion . . . . The small amount of erosion which it has suffered,'' he continues, ''seems hardly consistent with its correlation with the earliest glacial epoch.''

With these conclusions concerning the continuity of the glacial deposits in the Delaware and Susquehanna valleys ranging from the Columbia down, the special incredulity with which Mr. Cresson's testimony concerning the Claymont implement has been received falls to the ground. Probably the acceptance of the genuineness of that discovery would not imply an antiquity more than two or three thousand years greater than that which is implied in the genuineness of the Trenton implement.

THE ABORIGINAL QUARRIES AND SHOPS AT MILL CREEK, UNION COUNTY, ILL. BY DR. W. A. PHILLIPS, Evanston, Ill.

An exploration of the ancient quarries and shops at Mill Creek, Union county, Ill., was begun by the Field Columbian Museum, early in the present year, under the direction of Mr. Dorsey and the writer. Inspection of the place in December revealed a source of the large flaked implements of the Mississippi valley, which although previously noted in connection with mound explorations in the vicinity, has thus far received little attention. The material quarried was the grayish-brown chert commonly seen in the stone spades and hoes of Eastern Missouri and Southwestern Illinois, and of which specimens are frequently obtained from the St. Louis and New Madrid regions. While the locality may not be the only source of these remarkable implements, the remains are of con-

siderable extent, are also centrally placed with respect to the known distribution of products in the same stone and present several new features in the study of aboriginal quarrying and stone shaping.

The diggings comprise three groups of pits, covering an aggregate area of about twenty acres. The main site, consisting of two adjoining groups, is located on the wooded hills one mile west of the town, while a small group occupies a portion of a cultivated field two miles to the north. Flaking shops are numerous in the neighborhood; the largest of these, which covers several acres, is on the Hale farm near the county line.

The chert occurs in thin, flat nodules, of different shapes and sizes, and was obtained by digging through a deep layer of river clay to a mixed stratum of clay, sand, and free chert nodules which has resulted from the disintegration of the limestone underlying the region. A railroad cut at Weaver Hill, about 2½ miles north of Mill Creek, gives an excellent idea of the formation with which the ancient quarrymen had to deal. In the exposed section at this point the upper stratum of clay is 15 feet thick, the mixed stratum containing free nodules, 12 feet, and the underlying rock enclosing nodules, 9 feet to the bottom of the cut. Many of the hills near-by are of similar character.

A trench dug in the main quarry group and intersecting several pits was the chief work accomplished. Five ancient openings were encountered in the section which was 64 feet long and 24 feet deep at the lowest point reached, namely, the upper surface of the nodule-bearing stratum. The pits appear to have been narrow, deep holes, in the nature of shafts extending through the upper clay. Those which were left open when deserted by the ancient quarrymen, gradually acquired funnel-shaped mouths by caving in at the edges, and with the washing-in of additional material from the dump-piles were filled in the course of time almost to the level of the former surface. One of the intersected pits was found to have been filled while still fresh with excavated clay and nodules from the bottom of some neighboring pit, and was thus preserved in its original shape.

The tools recovered from the trench consisted of a number of stone hammers of peculiar type and a broken flaked-stone shovel. The hammers were provided with short handles, rudely shaped out of the stone, and presented a polished area on the flat side of the hammerhead, presumably from friction and long contact with the thumb while in use. The shovel was a thick, roughly-shaped tool of the general type commonly classed as agricultural implements; the wear, however, was not in the nature of a polished surface at the used end, but consisted mainly of a rounding-off of the edge. Other specimens of this type of implement were found on the lodge sites.

Refuse on the quarry sites was made up for the most part of broken nodules, irregular ends and pieces left from a selection of material suitable for flaking. Little flaking was done about the pits.

Flaking refuse and rejectage was found in large deposits on lodge sites, which are distributed in groups all about the neighborhood. The flakes

are of unusual length, thin and wide, and illustrate the remarkable quality of the stone even more forcibly than do the rejects and finished forms. The shape in which the stone occurs has also had much to do with the successful flaking of large and broad implements. The nodule generally presents a cross-section in the shape of a greatly elongated ellipse, which required the removal of only a few flakes in most cases to assume the form desired.

Many products, all of large size, are traceable in the rejectage, and fifteen or more specializations might be enumerated from the collections made in the vicinity. They include several varieties of spades and hoes, celts, chisels, and gauge-shaped tools, knives, etc. Some highly developed forms were finished by grinding on the flat, while an edgewise grinding is characteristic of all finished forms and effected that part of the implement which was designed to enter into the hafting. There is a notable absence of projectile points, awls and other common forms produced from quarry-shop blades derived from such centers as Piney Branch and Flint Ridge.

Tools recovered from the shop sites include numbers of flaking hammers of the ordinary type found throughout the country. They are principally of the same material as the stone worked. A few hammers with handles like those found in the trench also occur. Stones used for grinding are particularly abundant. Some specimens are of large size and marked with deep grooves from the edgewise grinding of implements as already described, while others are without grooves and were used for grinding on the flat. Many rejects served as grinding tools and blocks of sandstone and massive chert are among the materials thus used.

(The distribution of products was shown by a general map, and the location of the quarries and principal shop sites by a map of the vicinity of Mill Creek. Photographs and diagrams of the excavation and railroad cut were exhibited and specimens of rejectage, products, and tools.)

THE NATURAL DIATONIC SCALE. BY CHARLES K. WEAD.

This paper gives an account of the history of the three words in the title. In the mediaeval tables those hexachords involving the use of b-molle (b ♭) were called *molle;* those involving b-dur (b natural) were called *dur;* and those that did not involve either b, but applied Guido's six syllables to the letters in their natural order from A up, were called *natural* hexachords; the other things and names have lost interest for modern musicians, and the so-called "natural" series remains, and retains the name. "Diatonic" refers generically to those tunings of the intermediate strings of the Greek lyre, located between E and A, in which the strings were most "on the stretch," and so gave their highest tones, and specifically to the highest of these tunings, the one giving substantially the same succession as our E, F, G, A. "Scale" is not

found in use till after 1500; the old word for the series of sounds was *systema*; for the notes on paper, *diagram*. *Scala* at first referred to the tables of hexachords, then to notes arranged on lines and in spaces, and finally to the series of sounds, which is now the exclusive meaning.

ALLAN STEVENSON'S TRANCE. BY DR. ROLAND STEINER, Grovetown, Georgia.

Allan Stevenson told me the following story of his trance. He was out in the woods splitting rails, when he was approached by a big black dog, which told him to follow him. He refused to do so. He was then approached by a goat, and requested to follow him, but he again refused to do so. He was then approached by a white lamb, and requested to follow it. He followed the lamb, and it led him to the side of a large ditch and there told him that he must remain three days and three nights without either food or water. He lay there in a stupefied condition and an immense concourse of negroes assembled there from all quarters, out of curiosity, fear, and superstitious feeling of all kinds. At the end of three days, he arose from the ditch, went up to his cabin, bathed his face and hands, and called for something to eat. He said he had a message for me. He came over and told me that the Lord had sent him to me to have a large church built on my place. I told him that there was already a small church there that would answer all our purposes for the time being.

I asked him where he had been during his trance. He told me that he had been to the end of the world, and that it turned upon a spindle, which is like a grindstone, and the spindle is now nearly worn through, and that he expected it to break through at any time, and that there would be ruin and desolation on the earth and that the end of the world would come. He said he went to hell, and there were two big black dogs guarding the gate. He was told by his good spirit, which accompanied him, not to notice these dogs at all, and that his faith would keep him secure from any attacks by them. After he had passed the dogs, he went up to where the devil and his wife were. The devil was a very dark-skinned man and all shrunken. He had on a coat and had a long tail with a spear at the end of it, and his hands were like claws. The devil's wife was dressed in red and was shrunken and shrivelled like the devil. The devil asked him if he would like to see the burning pit, and he said he would, and his good spirit told him he would go with him. He went to the edge of the burning pit and said it was a big lake of fire and brimstone and was filled with the heads of sinners, that is, only the heads of the sinners were showing. The sinners were all trying to call on the name of the Lord, but could not say Lord, but said oui, oui, oui. He said he was almost suffocated by the fumes of the sulphur, and asked his spirit to take him away from that place. As he returned from the pit, the devil's wife

asked him if he would have something to eat. She had a pan in her hands and in it was something that looked like greens all mixed together, but the spirit told him not to touch any food in that region at his peril. The devil and his wife then left him and the spirit took him up the side of a high mountain and when he got to the top, he saw a large plain with a river running through it, and that river was the river Jordan. When he got to the river Jordan, two angels with white wings, were there, and one stood on either side of him and flew across the river Jordan with him. When he got to the other side, he saw that everything was as white as snow—he was in Heaven. It was the most beautiful place he had ever seen in all his life. They asked him if he wanted something to eat. The honey was as white as snow and the cake was as white as snow. They all sat down at a long table that seemed to reach into eternity. An angel asked him if he did not want to fly and see the country all round. They put a pair of wings upon him, but it was a long time before he could fly. They had to show him how, and when he would try to fly a few feet from the ground, he would fall back and they told him that he did not have faith enough. Afterwards he succeeded, through the assistance of these two angels and got away up in the air and flew around on the " superbs" of the Heavens. As far as he could see, there were angels, big and little, flying all around. There were gold houses, and silver houses, and so bright that he could hardly look at them. When he lit, as he called it, he woke up out of his trance.

This man then proclaimed to the country at large, that he was a prophet and sent in the name of the Lord to work miracles. He went into my negro quarter and saw a small child with a sore on its cheek. The child was brought to him to cure. He took some of the ashes out of his pipe and put it in the sore. I heard of it, and went down and had the ashes taken out and told him that he must not do such a thing as it would be liable to create a great deal of harm. He went to the Rocky Creek Church, in Burke County, and demanded of the colored ministers that they baptize him. They, seeing his frantic and excited condition, refused to do so. He announced on the following Sunday that he would baptize himself. On the following Sunday, he walked about two miles at the head of a large concourse of negroes, to the Rocky Creek Church, where there were assembled at least five thousand negroes. He went into the water, and lifting up his eyes to heaven, cried with a loud voice: "In the name of the Father, Son, and Holy Ghost, I baptize thee, Allan Stevenson."

A COMPARATIVE STUDY OF THE PHYSICAL STRUCTURE OF THE LABRADOR ESKIMOS AND THE NEW ENGLAND INDIANS. BY FRANK RUSSELL AND HENRY MINOR HUXLEY, Harvard University, Cambridge, Mass.

Geographical position of the Labrador Eskimos and their relations

with adjoining Indian tribes.—Prior to the year 1600 the Eskimos inhabited the southeastern coast as far as Mingan, on the Gulf of St. Lawrence. They were driven back to the Strait of Belle Isle at that time by the Indians who had obtained fire-arms from the French. At the present time the Eskimos are not found south of Hamilton Inlet.[1]

Turner[2] states that Inuit of pure blood do not begin to appear until the station of Hopedale is reached. Along that part of the coast the Moravian missionaries prevent the natives from coming in contact with the whites ; previous to the arrival of the latter the condition of hostility existing between the Algonquin tribes and the Eskimos probably kept the latter comparatively free from Indian blood.[3]

Material.—The Peabody Museum of American Archæology and Ethnology contains 44 crania of Labrador Eskimos ; from this collection 33 have been selected for study. The available series contains one skull, No. 2708, which was described by Dr. Jeffries Wyman[4] as a " cranium from Dr. Parks"; 3 were obtained from a cave on the coast by Gloucester fishermen ; 29 were collected by Mr. J. D. Sornberger, at Okkak, Hebron, and Hopedale. For comparison with these, crania from North Greenland were studied, 9 from Herschel Island on the Arctic Coast of America, west of the mouth of the Mackenzie, collected by the writer : and 200 skulls belonging to three widely separated stocks of American Indians. Only one of the Indian groups need be considered in this abstract—that from the Atlantic Coast.

Published descriptions of Labrador crania.—Blumenbach gave a brief account of five skulls from Nain, Labrador. Virchow[5] has reviewed this description and added that of one skull from Hebron. Duckworth[6] recorded twenty measurements and indices taken upon four skulls from Hopedale and Cape Ailek. In 1881 more or less detailed observations had been made upon 159 crania of the Eskimos of Danish Greenland. Bessels'[7] investigations upon the " Itanese," or North Greenlanders made a total of 260 Greenland crania described.

Hamy[8] states that the average cranial index of 15 Western Eskimo skulls is 76, of 159 Eastern—71.5. According to Davis[9] the cranial index of the West Greenlanders is 71 (average of ten), and of the Inuit of " Northwest America " —72 (average of four).

The determination of the relation of the physical type of the Labrador Eskimos to those of the Greenland and Western Eskimos may shed some

[1] A. P. Low, Ann. Rept. Can. Geol. Sur., 1895, Rept. L, p. 51.

[2] Lucien Turner, Eleventh Ann. Rept. Bur. Eth., p. 176.

[3] This opinion is founded upon the statements of Ellis, A Voyage to Hudson's Bay, p. 182 ; Kohlmeister and Kmoch, Jour. of a Voy. from Okkak, 1814, p. 57 ; and Hind, Labrador, Vol. II, p. 101.

[4] Observations on crania, Boston, 1868, p. 22.

[5] Zeit. für Eth., Verh. Bd. XII, s. 255.

[6] Jour. Anth. Inst., Vol., XXV, p. 72.

[7] Bessels, Archiv. fur Anth., Bd. VIII. s. 120.

[8] Bull. Soc. d' Anth., series 3, T. IV, p. 18.

[9] Thesaurus Craniorum, p. 223.

light upon the problem of the place of origin[1] of the whole group. The Eskimos are said to have once occupied the New England coast; may we not learn, also, from an examination of the physical characters of the two groups to what extent intermixture of Eskimos and Algonquins has taken place?

Viewed from above, the crania of the Labrador Eskimos vary in outline from an elongated pentagon to a narrowed ellipse. The females are more uniformly pentagonal than the males; the sides of the female crania are more nearly vertical than those of the males and their transverse diamters are proportionately greater at all points except in the bi-auricular region.

The high, narrow, or carinate vertex is a characteristic feature of the Eskimo skull; that the Indian crania are not of this form may be shown by a diagram of the transverse arch or by a comparison of the transverse diameters compared with the maximum taken as 100 as follows:

	Males.		Females.	
	Labrador.	New England.	Labrador.	New England.
Bi-asterial	80.2	79.5	78.6	79.1
Bi-auricular...............	88.1	88.2	85.9	86.1
Bi-stephanic	56.2	77.3	66.9	76.
Inter-pterion.............	79.6	79.4	78.8	78.1
Minimum frontal.........	68.9	68.2	69.5	67.8

All the skulls are phaenozygous and most of the zygomatic arches have been preserved. The temporal crest is feebly indicated in all and the bistephanic breadth, cannot be determined with accuracy. The lines upon the prominent occiput are not pronounced and the inion is not well developed in most cases.

Wormian bones.—Several wormian bones occur in the lambdoidal suture of one skull but elswhere in this series they are either single or in groups of two or three. There is but one example of an epactal bone, as the per cent.—three—corresponds to that determined by Anutchin for the Mongolians and as his investigations showed that this anomaly occurred in nearly three times this proportion of Indian crania (Peruvians excepted)further observations are desirable.

Sutures.—The condition of the sutures and the percentage of occurrence of certain foramina are susceptible to statistical treatment and are characteristic of racial or, perhaps, culture groups. The lambdoidal suture is complex in all but two of the skulls of the Labrador series. More or less evident traces of the lower portion of the metopic suture remain in ten skulls. The inter-nasal suture is closed in half the series.

The *infra-orbital suture* occurs with equal frequency in the two sexes.

	Present. Per cent.	Wanting. Per cent.
Labrador......................	79.8	20.7
New England	56.6	43.4

[1] Dr. Boas regards the region west of Hudson Bay as the place of origin of the Eskimos(Die Sagen der Baffin Land Eskimos, Verh. Berlin Gesell., Bd. xvii, s. 165). Dr. Rink placed the center of distribution in Alaska (The Eskimo Tribes, p. 4).

Anutchin has shown that the length of the *spheno-parietal suture* varies to a considerable extent among the principal groups of mankind. Comparing the two groups we have :

	Average length.	Range.	Per cent. less than 8 mm.	No. measured.
Labrador	11.	3 to 21	13.7	51
New England	13.8	2 to 26	4.3	47
California	10.7	2 to 26	14.9	235

The per cent. of those less than 8 mm. in length corresponds closely with that given by Anutchin[1] for the Mongols.

Foramina.—The post-condylar foramen exhibits the following percentage relationships :

	Present on both sides. Per cent.	On left.	On right. Per cent.	Wanting.
Labrador	90.	0	10.	0
New England	70.9	12.9	12.9	3.3

Parietal foramina.—

Labrador	43.3	13.4	20.	13.4
New England	22.9	17.1	17.1	42.9

In one skull a horizontal spine projects backward 6 mm. from the basion. In the New England series there is one example of the union of the atlas with the base of the skull.

Posterior nasal spine.—This spine is never of large size in the Labrador skulls; it tends to become bifurcated in some cases, in others the projection is very slight and the posterior margin of the palate is deflected downward. In general, it is less acute than in the New England crania.

Hard palate.—The palate is of a low type among the Eskimos. It is U-shaped in 11.5 per cent.

The measurements now taken upon the hard palate vary to such an extent, and the manner in which they are taken is so seldom stated that but a small proportion of the recorded data are available for comparison. The breadth is taken at almost any point between the second incisors and the end of the arch. The length may include the posterior nasal spine, introducing an apparently unnecessary element of variation, and the thickness of the alveolar arch anteriorly, which, as it is not included in the transverse measurements ought, to be consistent, to be omitted in ascertaining the length.

Notwithstanding the remarkably shallow palate of the Labrador crania the incisors are vertical.

Alveolar hyperostosis.[2]—This bony outgrowth occurs in 9 of the 18 jaws

[1] Anutchin's results were epitomized by M. C. Merejowsky in a review. *Sur quelques anomalies du crane humain et leur frequence dan les races. Revue d'Anth.*, série 2, T. V., p. 359.

[2] Dr. Harrison Allen called attention to this character in Eskimo crania in his Toner Lecture. Smith: Mis. Contr. 1890, p. 13. See also his Crania from the Mounds of the St. John's River, p. 427.

of the collection. The enlargement varies from a slight ridge of even thickness to a mamillated alveolus that projects a distance of 8 millimeters from the lingual margin of the second premolars. Hyperostosis is quite marked in one young female skull with the teeth but little worn ; it occurs also in one male skull in which the teeth exhibit less than the usual amount of wear. These two examples lend support to the supposition that the anomaly results from other causes than strain upon the teeth. It occurs in 50 per cent. of the Labrador skulls, and, in a less marked degree in 11.5 per cent. of those from New England. It is present in 14.7 per cent. of 225 jaws from the Stone Graves of Tennessee, while in 78 jaws from California scarcely a trace of hyperostosis is to be found.

The nasal aperture.—The terms proposed by Dr. Allen[1] to designate the forms of the inferior margin of the nasal opening are here used. Unfortunately, the anterior nasal region is a very variable one and though the series is small it may be arranged in an almost perfect gradation from the sharp and angular form to the guttered. They are grouped as follows:

	Labrador.			
	Males.	Females.	Both sexes.	Per cent.
Macrolophic ...	8	4	12	46.1
Microlophic ...	4	6	10	38.6
Analophic	.	4	4	15.3

Capacity.—All the skulls that were not too fragile were gauged with shot according to Broca's method. After seeing the Poll[2] apparatus for gauging cranial capacity with water in successful operation in Berlin Iregauged the Labrador skulls by that method.[3] Two characteristics of the Poll method may be accepted as proved—uniformity and the elimination of the personal equation. Ease of manipulation and freedom from dust also commend it. Does the lenticular rubber bag enter the sella turcica and the fossa over the cribriform plate so that the entire space is filled and the exact capacity ascertained ? To test this I have taken a skull from the laboratory and cut sections from the body of the sphenoid and from the floor of the anterior fossa so that the sella turcica and the space on either side of the crista galli were exposed to view. On filling, the bag completely closed the pituitary fossa and extended into the artificial opening made by the saw ; anteriorly, an unusually high crista galli prevented the bag from filling a minute space that seemed too insignificant to justify special attention for accurate measurement.

Fifteen crania were gauged, each ten times ; the minimum range of variation in a single skull was 5 cc. and the maximum 8 cc. As might

[1] Harrison Allen: Crania from the Mounds of the St. John's River, Fla., p. 419.

[2] See the Zeit. fur Eth. Verh., Bd. xxviii, s. 614.

[3] A brief account of this trial was published in the *American Anthropologist* of February, 1898. In the June number of the *Anthropologist* Dr. Washington Matthews reviewed the experiments that had been made in gauging cranial capacity with water, and questioned the accuracy of the Poll method. At the time my one-page article was published I was quite well aware of the painstaking investigations of Dr. Matthews as well as of most of those which had gone before but did not wish to give a history of the subject.

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have been expected, the results obtained in gauging a bronze-skull were more variable and the average was somewhat less than the actual capacity, owing to the air being retained between the walls of the rubber bag and the air-tight skull.

ESKIMO SKULL, NO. 47993.	BRONZE SKULL.	
Corrected result with water.	With water.	With shot.
1–1445	1292	1330
2–1444	1294	1342
3–1443	1292	1344
4–1446	1289	1336
5–1445	1297	1334
6–1445	1291	1340
7–1447	1290	1335
8–1445	1294	1344
9–1445	1294	1345
10–1445	1291	1346
Range, 1443–1447	1289–1297	1330–1346
Average, 1445	1292.3	1339.–6–excess, 40.8

Of the 15 Eskimo crania, the range of variation in ten trials in 3 crania amounted to 5 cc. ; in 6, to 6 cc. ; in 4, to 7 cc. ; in 2, to 8 cc.

Cranial index.—In this important index the group relations are most interesting and a few other averages are given for comparison :

```
(Greenland 76 crania..................... 71.0)
(North Greenland, Bessels............... 71.4)
Labrador .................................. 71.9
New England .............................. 74.6
Herschel Island ........................... 74.6
(New England, Carr, 38 males)............ 76.1
   "       "       "    28 females)......... 75.2
```

The Labrador females are slightly more brachycephalic (72.3) than the males (71.9). The series falls in the following groups :

	Labrador				Herschel Island.
	Males.	Females.	Doubtful.	Per cent.	
Hyperdolichocephalic ...	5	3	1	29.0	0
Dolichocephalic	5	9	2	51.6	5
Mesaticephalic	3	3	0	19.4	4

Vertical index.—Here again the New England series approaches that from Herschel Island more closely than that from Labrador :

	Labrador.	Herschel Island.	New England.
Vertical index	71.9	73.5	74.5
Breadth-height	100.0	96.5	98.5

Index of foramen magnum.—In the form of the foramen magnum the Eskimos are distinctly separated from the Indians—to a greater extent than the more dolichocephalic skull would lead us to expect. In the Indian series the index is higher in the males; in the Eskimo group the sexual difference is reversed.

As a whole the capacity of the Labrador crania is small according to the classification of Broca:

Medium capacity 1
Small 13
Microcephalic 1

Number of crania ·	Labrador.			Herschel. Island. 2 males. 7 females.
	Males. 6	Females. 6	Both sexes. 14	....
Average	1410.2	1325.0	1354.2	1384.3
Range............	1335.4–1460.3	1145.5–1452.5	....	1194.3–1557.7

The sexual difference, 85.3 cc., is about that found among " natural races" in general.

Naso-malar index.—The degree of prominence of the root of the nose of the Labrador Eskimos (index 106.2) is approximately that given by Oldfield Thomas for the 9 Mongols measured by him (105.9), the average negro having an index of about 107. The average naso-malar index of the New England skulls is 109.3.

Orbital index.—A striking difference is seen in the shape of the orbit especially between the males in the two groups :

	Males.	Females.	All.
Labrador	87.5	87.4	87.2
New England	81.0	85.5	83.4
Herschel Island.............	...	(6)	86.3

The slight sexual difference in the Eskimo series is noteworthy. The percentage distribution is as follows :

		Labrador. Per cent.		New England. Per cent.
Microseme	7	22.6	17	48.6
Mesoseme..........	12	38.7	10	28.6
Megaseme	12	38.7	8	22.8

Nasal index.—The nasal aperture is much narrower in Eskimo than in Indian crania ; the sexual difference is also well marked throughout.

	Males.	Females.	All.	Leptor- rhinian. Per cent.	Mesor- rhinian. Per cent.	Platyr- rhinian. Per cent.
Labrador.....	42.9	44.1	43.5	83.9	16.1	0
New England.	47.5	49.9	48.7	44.4	30.6	25.0

Facial angle.—Oriented with reference to the German horizontal plane and measured with the Ranke goniometer the Eskimo crania are found to be slightly more prognathous than those of the Indians, though racial and sexual differences are slight. This is also shown by the alveolar index though it does not coincide very closely with the facial angle.

Facial index.—Owing to the broken zygomatic arches in the New England series the number of cases in which this index could be reckoned was reduced to too small a number for the average to have any value.

1 Jour. Anth. Inst., Vol. xiv, p. 333.

AVERAGES OF MEASUREMENTS AND INDICES.

	Males.		Females.	
	Labra-dor.	New England.	Labra-dor.	New England.
Capacity	1410.2	1265.8	1325.	1146.5
Glabello–occipital length	189.2	187.6	179.6	176.2
Maximum breadth	136.2	138.6	129.9	134.4
Bi-asterial breadth	109.3	110.2	102.2	106.3
Bi-auricular breadth	120.	122.3	111.7	115.7
Bi-stephanic breadth	76.5	107.1	86.9	102.1
Inter-pterion breadth	108.5	109.9	102.4	105.
Minimum frontal breadth	93.9	94.6	90.4	91.2
Bi-zygomatic breadth	138.1	140.	128.5	129.
External bi-orbital breadth	135.6	107.	102.	101.7
Internal bi-orbital breadth	99.9	101.1	95.	97.
Bi-jugal breadth	118.1	118.4	110.7	111.1
Bi-maxillary breadth	100.9	101.	95.6	94.6
Bi-alveolar breadth	64.1	65.8	61.2	63.7
Maxillary length	54.5	55.5	51.1	54.3
Basi-alveolar length	100.9	103.8	96.8	100.
Basi-nasal length	104.1	106.9	97.6	101.1
Basi-bregmatic height	136.	136.5	130.4	133.7
Basion-obelion	129.7	131.4	125.5	128.4
Basion-lambda	119.5	118.6	113.2	116.4
Length of foramen magnum	37.7	36.6	35.8	35.9
Breadth of foramen magnum	30.6	31.4	29.6	30.1
Malar height	50.6	48.	47.8	44.9
Naso-alveolar height	72.3	69.6	65.5	67.1
Spino-alveolar height	20.2	21.1	17.2	20.
Naso-mental height	125.	123.6	113.3	115.3
Orbital breadth	42.4	42.5	39.7	39.6
Orbital height	37.1	34.4	34.6	33.9
Orbital depth, optico-nasion	54.5	53.7	51.2	50.3
Bi-dacryc breadth	18.2	20.8	18.8	19.3
Nasal height	52.7	52.4	49.4	50.3
Nasal breadth	22.7	24.8	21.8	25.4
Palatine length	49.6	49.6	46.5	48.1
Palatine breadth, canines	24.2	26.1	24.	25.4
Palatine breadth, second molars	40.8	42.1	38.8	40.8
Dental length	38.1	40.5	38.3	39.1
Height of choanae	26.7	25.	23.2	23.5
Breadth of choanae	29.9	28.	27.9	28.2
Arcs :				
Naso-malar	107.1	111.3	101.3	106.4
Frontal arc	128.2	127.5	126.6	122.8
Parietal	123.6	125.	122.	121.9
Occipital	120.9	120.1	115.2	115.3
Total sagittal	372.8	363.5	363.7	359.6
Maximum transverse	312.	318.9	304.5	311.1
Total transverse	438.1	438.7	421.3	422.8
Supra-auricular	301.6	306.5	292.9	297.4
Pre-auricular	239.4	241.2	231.3	228.5
Total horizontal	520.6	517.9	497.8	495.5

INDICES.

	Males.		Females.	
	Labrador.	New England.	Labrador.	New England.
Cranial	71.9	74.1	72.3	75.1
Vertical	71.9	73.1	72.5	75.6
Breadth-height....	100.1	98.1	100.4	98.9
Stephano-zygomatic	81.3	131.5	96.1	119.4
Upper facial, Kollman	51.9	51.7	51.1	53.8
Total facial......................	92.6	90.1	85.6	90.8
Naso-malar	106.5	109.	105.9	109.
Orbital	87.5	81.	87.4	85.5
Nasal.............................	42.9	47.5	44.1	49.9
Maxillary.........................	117.9	118.2	120.	117.9
Palatal	83.6	83.7	83.2	83.2
Dental............................	36.7	37.3	39.1	38.7
Alveolar	96.6	96.8	97.	98.5
Facial angle, Ranke...............	83.1	84.1	83.7	83.5
Relations of arcs :				
Frontal-total sagittal.............	34.5	34.1	34.7	34.
Parietal-total sagittal	33.1	33.6	33.2	33.8
Occipital-total sagittal	32.4	32.1	31.6	32.1
Pre-auricular-total horizontal.......	45.6	46.5	46.4	46.1
Supra-auricular-total transverse	69.	69.7	69.4	70.3

AVERAGE MEASUREMENTS OF THE MANDIBLE.

	Labrador.	New England.	Labrador.	New England.
Bi-condylar breadth	121.4	129.0	118.6	117.6
Bi-gonial breadth.................	109.3	102.3	103.2	103.0
Symphysial height................	34.0	34.1	30.8	31.6
Malar height.....................	29.3	30.9	27.0	26.9
Ramus height....................	56.7	62.0	52.5	47.5
Ramus breadth...................	43.3	37.6	36.9	33.9
Gonio-symphysial chord	91.6	93.6	87.0	89.5
Condylo-coronoid chord..........	38.1	37.0	36.9	34.4
Bi-gonial arc....................	200.2	206.2	198.7	191.3
Gonio-zygomatic index...........	77.9	72.3	82.2	78.1
Mandibular index................	92.2	90.2	87.7	85.5

LONG BONES.

Lack of space prevents the consideration of the greater part of the accumulated data relating to the dimensions and proportions of the long bones; femur, tibia, humerus, and radius were measured, the series comprising the

Labrador Eskimos, and the Indians [of Tennessee, California, and Massachusetts.

Femur.

Maximum Length.

	Male.				Female.			
	Num-ber.	Maxi-mum.	Aver-age.	Mini-mum.	Num-ber.	Maxi-mum.	Aver-age.	Mini-mum.
Eskimos	16	442	425.8	403	5	390	388	386
Tennessee......	51	479	442.1	397.5	48	444.5	442.8	369
California	18	462	434.3	399	29	453	406.1	370
Massachusetts ..	19	503	469.5	420	12	455	422.3	401

Index of Shaft at Middle.

	Male.				Female.			
	Num-ber.	Maxi-mum.	Aver-age.	Mini-mum.	Num-ber.	Maxi-mum.	Aver-age.	Mini-mum.
Eskimos	19	120.4	108.8	88.5	10	122.2	109.0	101.9
Tennessee	52	129.4	109.2	86.2	48	123.4	104.1	77.1
California	20	134.1	117.5	98.1	29	128.6	112.2	100.0
Massachusetts ..	23	134.6	114.1	98.2	16	120.0	109.8	104.2

In the males, The Tennessee Indians closely approach the Eastern Eskimos, and in the females the Tennesseans have a smaller average index than the Eskimos. Between the Eskimos and the Massachusetts and California Indians, there is a considerable difference. The sexual difference is distinct, although it is not found in the Eskimos.

Index of Shaft at Subtrochanteric Region.

	Male.		Female.	
	Number.	Average.	Number.	Average.
Eskimos	18	81.1	10	79.9
Tennessee	52	79.9	48	75.3
California	20	76.5	29	76.2
Massachusetts	22	74.0	16	70.4

The racial difference is well marked in both male and female, and in every case, the males have a higher average index than the females.

Popliteal index : Taken 4 cms. above the anterior superior articular border of the external condyle.

	Male.		Female.	
	Number.	Average.	Number.	Average.
Eskimos	19	73.0	9	77.6
Tennessee	52	81.0	48	77.3
California	19	78.2	29	73.8
Massachusetts	22	79.1	16	74.5

Although larger in the Eskimo, the popliteal index appears to be somewhat smaller in the female than in the male. In the males, the racial difference is well marked. This difference is not, however, seen in the females.

Angle of Neck and Shaft.

	Male.		Female.	
	Number.	Average.	Number.	Average.
Eskimos	16	129.9°	8	129 2°
Tennessee	51	123.2°	48	123.9°
California	20	122.4°	29	121.6°
Massachusetts	23	119.8°	14	120.9°

The angle of the neck and shaft is practically the same in the two sexes. There is, however, a marked racial difference.

Angle of Shaft from Vertical.

	Male.		Female.	
	Number.	Average.	Number.	Average.
Eskimos.............	14	9.8	5	10.2
Tennessee	52	8.2	48	9.6
California	18	9.7	29	12.2
Massachusetts	17	9.9	12	11.5

In every case, the angle of the shaft from the vertical is larger in the female than in the male. This is satisfactorily explained by the broader pelvis of the female.

Angular index : The relation between the angle of the neck and shaft, and the angle from the vertical is expressed by the angular index which is angle of shaft from vertical $\times$ 100.

Angle of Neck and Shaft.

	Male.		Female.	
	Number.	Average.	Number.	Average.
Eskimos	13	7.7	5	8.0
Tennessee...........	51	6.6	48	7.9
California...........	18	7.9	29	10.0
Massachusetts.......	17	8.3	10	9.8

Like the angle of shaft from the vertical, the angular index is, of course, greater in the female.

Angle of Torsion.

	Male.				Female.			
	Number.	Maximum.	Average.	Minimum.	Number.	Maximum.	Average.	Minimum.
Eskimos	16	28°	18.6°	5°	6	28°	23.8°	20°
Tennessee....	50	38°	21.2°	9°	47	41°	23.3°	—5°
California	18	37°	27.6°	14°	29	43°	30.3°	10°
Massachusetts	14	28°	22.3°	15°	10	46°	29.3°	11°

In every case the female shows a greater angle of torsion than the male. There is also a considerable racial difference.

TIBIA.

Length : Measured from the condylar surfaces to the extremity of the internal malleolus.

	Male				Female.			
	Number.	Maximum.	Average.	Minimum.	Number.	Maximum.	Average.	Minimum.
Eskimos	15	387	345.3	326	9	327	313.9	297
Tennessee	36	398	359.3	331	33	369	340.7	302
California	20	381	363.6	326	26	374	336.4	295
Massachusetts	13	406	390.7	353	13	380.5	357.9	343

Index of Shaft at Middle.

	Male.				Female.			
	Number.	Maximum.	Average.	Minimum.	Number.	Maximum.	Average.	Minimum.
Eskimos	16	88.5	75.5	64.3	11	76.0	72.6	63.4
Tennessee	36	78.7	64.8	53.7	33	86.3	68.7	55.3
California	20	73.7	65.6	56.7	26	80.8	70.0	57.1
Massachusetts	14	75.9	65.4	58.3	15	70.8	63.8	55.2

By Sergi's classification, the Eskimos would be enknemic, above 71, whereas the Indians would be platycnemic, under 66, in the males and the Massachusetts females, and subplatycnemic, 66-71, in the Tennessee and California females.

Angle of Torsion.

	Male.				Female.			
	Number.	Maximum.	Average.	Minimum.	Number.	Maximum.	Average.	Minimum.
Eskimos	12	23°	14.6°	4°	9	28°	17.1°	2°
Tennessee	34	44°	26.4°	13°	33	38°	27.4°	15°
California	19	44°	29.3°	13°	26	50°	29.7°	12°
Massachusetts	14	44°	27.9°	13°	13	46°	26.2°	15°

Except in the Massachusetts Indians, the females show a slightly greater angle of torsion than the males. The difference between the Eskimos and Indians is very marked.

Curve of the external condylar surface : Following Thomson's[1] classification of the curves.

	Male.		Female.	
	Number.	Average.	Number.	Average.
Eskimos	13	1.4	9	1.8
Tennessee	36	2.3	33	2.9
California	19	2.5	26	2.9
Massachusetts	14	2.7	15	3.6

The determination of the curve is necessarily somewhat inaccurate, but is of sufficient accuracy to show a well-marked racial difference between the Eskimos and Indians. The female has, in every case, a somewhat greater curve than the male.

HUMERUS.

Maximum Length.

	Male.				Female.			
	Number.	Maximum.	Average.	Minimum.	Number.	Maximum.	Average.	Minimum.
Eskimos	10	312	295.6	282	9	304.5	287.1	266
Tennessee	44	350	317.9	286.5	37	324.5	297.3	264.5
California	15	321	308.2	276	20	322	288.2	266
Massachusetts	11	346	335	316	13	311.5	295.8	279

[1] Jour. Anat. Phys.,Vol. xxiii, p. 616.

Angle of Torsion.

	Male.				Female.			
	Num-ber.	Maxi-mum.	Aver-age.	Mini-mum.	Num-ber.	Maxi-mum.	Aver-age.	Mini-mum.
Eskimos	7	145°	136.4°	130°	8	167°	148°	121°
Tennessee	43	156°	139.9°	123°	37	170°	145.1°	125°
California	15	183°	147.8°	121°	19	175°	154.3°	140°
Massachusetts .	11	153°	144.2°	133°	11	167°	153.6°	145°

In every case the female presents a greater average angle of torsion than the male. The Eskimo has the lowest angle, except in the female, where the Tennessee Indians have an angle lower by 2.9°.

RADIUS.

Length.

	Male.				Female.			
	Num-ber.	Maxi-mum.	Aver-age.	Mini-mum.	Num-ber.	Maxi-mum.	Aver-age.	Mini-mum.
Eskimos	5	230.5	219.5	209.5	7	211.5	202.6	191
Tennessee	28	268.5	247.5	222	35	247.5	230.6	198.5
California	10	249	246.1	229.5	18	235	223.9	208
Massachusetts	13	276	262.1	244	8	251	236.3	220

RELATIONS OF LONG BONES TO EACH OTHER.

Humero-radial Index.

	Male.	Female.	Race.
Eskimos	74.2	70.6	72.4
Tennessee	77.9	77.6	77.7
California	79.9	77.7	78.8
Massachusetts	78.4	79.9	79.0

In this index there is a considerable difference between the Eskimos and Indians. In fact, there is a slightly greater difference here than between the Europeans, whose index is 74, and the Negroes whose index is 79.[1]

Femoro-tibial Index.

	Male.	Female.	Race.
Eskimos	81.1	80.9	81.0
Tennessee	81.3	82.5	81.9
California	83.7	82.8	83.2
Massachusetts	83.2	84.8	84.0

Here the Eskimos stand at the end of the series, but the difference between them and the Indians is not so marked as in the humero-radial index. Among the Indians there is a considerable variation, the Tennessee Indians closely approaching the Eskimos.

Intermembral Index.

	Male.	Female.	Race.
Eskimos	66.8	69.8	68.3
Tennessee	70.6	70.1	70.3
California	69.5	69.0	69.2
Massachusetts	69.4	68.2	68.8

[1] Quain's Anatomy, Vol. 11, Pt. 1, p. 99.

Here again the Eskimos are at the end of the series, but are closely approached by the Massachusetts Indians.

ANOMALIES.

FEMUR.

Third Trochanter.

	Present. Per cent.	Doubtful. Per cent.	Absent. Per cent.
28 Eskimos	0	3.6	96.4
210 Tennessee	7.1	28.6	64.3
47 California	0	27.7	72.3
47 Massachusetts	4.3	31.9	63.8

Hypotrochanteric Fossa.

	Present. Per cent.	Doubtful. Per cent.	Absent. Per cent.
28 Eskimos	7.1	25	67.9
207 Tennessee	9.2	26.1	64.7
48 California	8.3	29.2	62.5
43 Massachusetts	4.7	9.3	86.0

TIBIA.

Anterior Facet on Inferior Articular Surface.

	Well marked. Per cent.	Poorly marked. Per cent.	Absent. Per cent.
25 Eskimos	8	4	88
172 Tennessee	21.5	26.2	52.3
45 California	40	35.6	24.4
31 Massachusetts	54.8	19.4	25.8

As Thomson has pointed out, the occurrence of the anterior facet corresponds with the degree of curvature of the external condylar surface. By a comparison with the table showing the curve of the external condylar surface, this will be found to be true in the case of the Eskimos and Indians.

Two male tibiæ, belonging to the same skeleton, were found, one normal, the other having a diseased inferior articular surface, which must have rendered walking impossible. The diseased one had a very slender and smooth shaft, the interosseous ridge not being marked in the least, presumably owing to the lack of development of the muscles.

HUMERUS.

Supratrochlear Foramen.

	Male.		Female.		Right.		Left.	
	Number.	Present. Per cent.	Number.	Present. Per cent.	Number.	Present. Per cent.	Number.	Present. Per cent.
Eskimos	15	0	11	63.6	11	27.3	15	26.7
Tennessee	58	29.1	38	47.4	49	26.5	47	46.7
California	15	6.7	21	4.8	19	10.5	17	0
Massachusetts	14	0	15	26.7	14	21.4	15	6.7

The series of humeri was too small to give satisfactory results, except in the Tennessee group where the supratrochlear foramen occurs more frequently in the female (47.4 per cent.) than in the male (29.1 per cent.) and on the right and left sides in the proportion of 26.5 to 46.8 per cent. Humeri exhibiting this peculiarity appear to be more abundant in certain localities rather than throughout tribal groups.

CONCLUSION.

In almost every measurement and index a considerable gap has been found between the Labrador Eskimos and the Indians. The difference is sufficiently marked to warrant us in maintaining that the skeleton shows no evidence of intermixture of these peoples. In fact the Tennessee Indians resemble the Labrador Eskimos far more closely than do the Algonquin Indians of Massachusetts ; and in cranial characters the New England tribes resemble not the adjoining Eskimos of Labrador, but the distant Eskimos of the Mackenzie[1] region.

PREHISTORIC SETTLEMENT, BIG KIOKEE CREEK, COLUMBIA COUNTY, GEORGIA. BY DR. ROBERT STEINER, Grovetown, Ga.

Upon a beautiful plateau on the west side of Big Kiokee creek, a mile above its confluence with the Savannah river, is located this prehistoric settlement. The elevation of the village site above the immediate lowlands of the Savannah river is about 100 feet. The area embraced by the settlement is about sixty acres; 27 springs have been counted in it. From the base of the hill occupied by the settlement, to the Savannah river on the north side, stretches a lowland of over a hundred acres, which was unquestionably cultivated by the Indians, as many hoes and spades of chert are found scattered over the surface. Upon the east side of the settlement skirting Kiokee creek on either side, are large swamps of an almost tropical vegetable character, containing canes or reeds of 30 feet in height, immense live oaks and other trees ; nut-bearing trees of every variety abound, whilst native wild grapes, berries, and fruits are found on every hand. These swamps abounded with game of all kinds : bear, buffalo, deer and other small animals, besides birds of every southern variety. The creek and river teemed with ·fish and mussels and waterfowl. Altogether, Nature contributed in every way to render this spot an Eden of happiness for the Red Man; the forest furnished him with food and clothing through its game, fruit, and nuts; the river and creek gave him fish and waterfowl, also mussels, from which he had not only obtained an edible oyster, but pearls for adorning his person. The village gradually merged into the base of Burt Mountain, with an altitude of 600 feet above the surrounding country, from which a lookout could be had to guard

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[1] On the hostile relations existing between the Mackenzie River Eskimos and the adjacent Indians. See British Arctic Blue Book, 1853, Vol. L, p. 54 ; Sir John Richardson, Arctic Searching Expedition, p. 133 ; Masson, Les Bourgeois, Vol. I, p. 95.

against surprise of an enemy. Abundance of reed canes furnished him
with stems for arrows, arrow-points (when hardened by fire,) pipe-stems
and knives, air- or blow-guns—the cane can be put to many uses; the cane
even serves for spearing fish or game. The earth furnished the material
for his weapons, his pottery, and his ornaments ; in minerals, there is
schist, the various forms of quartz, and red clay.

Within the village area near Kiokee creek is a quarry of steatite, from
which were manufactured many useful objects, such as mortars, pitted
stones, sinkers, pipes, rubbing stones, ornaments. The material can be
heated, and put in pot to boil water or rubbed over hides to take off any
irregularities, could also be used in polishing wood or making plates to
bake bread upon, and it being a Trust, in the hands of one tribe or na-
tion, could be an industry of intertribal communal interest in the way of
barter and exchange. Many of the objects were obtained in the rough
to be taken away and finished according to the fancy of the purchaser.
In the United States National Museum, I have many of these rude forms.
Many pitted stones are found here of all forms and sizes, with pits on
both sides, or on three sides, sometimes on one side, frequently with a
large excavation to be used as a mortar on one side and the reverse side
covered with pits ; their use seems to have been in cracking nuts. I
found *en cache* 202 of these pitted objects. The hammers, large and
small, and the picks for getting the material from the quarry, are all
found here. The work of preparing the material, roughing it, was con-
fined to the quarry. Quartz crystals of various sizes were used as tools
in its manufacture, as well as jasper, chert, and quartz knives, and rub-
bing stones of various dimensions and forms. There are also many open-
air workshops where chipping was carried on, as they are found scattered
over the village site always near a spring.

Along the southern bank of the Savannah river, extending for one
mile, from the mouth of Big Kiokee creek to the mouth of Little Kiokee
creek, is one continuous shell heap. It is separated from the village
site by Big Kiokee creek and is distant a mile from its nearest point. In
some places the shell heap is fifteen feet wide and ten feet deep—con-
taining broken shells, bones of men and animals. The manner in which
the human bones are found in their relation to the animal bones and
shells leads me to believe that at one time the people were cannibals. All
kinds of objects were used to open the shells—broken arrow-points, flakes,
and rough hammerstones. I have not thoroughly examined the shell
heaps, but shall do so at an early day.

At the confluence of Kiokee creek, there is an embankment nearly six
or seven feet high, that contains an area of almost two acres, continuing
back from the creek in a semicircular form and returning to the creek,
the line of the semicircle being on the creek. It may have been an en-
closed fortification strengthened by high pallisades.

The village site was admirably located high above the lowlands of the
Savannah river and Big Kiokee creek, with fresh pure water from the
springs, and seems to have been occupied for a long period of time and

to a very late date. The steatite quarrying must have been a paying industry and judging from the material of which many objects were made, the intertribal barter was extended to long distances, as there are found objects of flint, chalcedony, jasper, moss-agate, serpentine, and other minerals that are foreign to that section. I have found arrow-points, knives and hairpins of the same material as those found at the Etowah mounds, a distance of 250 miles. The material common to the country seems to have been used for all needed objects, such as knives, spears, and arrow-points. I have found crystal arrow-points and knives clear as water. I have no whole pots, but many sherds, showing the handiwork of the potter, and they compare favorably with the best southern pottery. Many unique objects are found there, *e. g.*, a pipe made from the antler of a deer; there are also many objects of European manufacture, such as pipes and beads ; a pipe with the British lion on the bowl is interesting from the fact that it shows the British occupancy of the country.

A few words on the character of the chipping and the objects may not be out of place. Quartz and chert were the materials from which most of the chipped objects were made. A large quartz quarry two miles from the village site seems to have been the point from which they obtained the material for their spears, arrows, knives, etc., etc. It was easily worked and served every purpose. The usual forms of arrow- and spear-heads are found upon the village site, but with certain peculiarities—the triangular form is present in large numbers, from 1¼ to 3½ inches long, made of quartz, flint, chalcedony, but few specimens of chert. Peculiar forms of asymmetric knives are met with (sufficient in number to warrant their not being simply freaks of fancy) some lance heads closely resembling the Copenhagen type, though not so highly finished, a peculiar form of spear-head suitable for spearing fish. The bevel-edged type is abundant ; many of which are serrated, as is the case with many triangular specimens. There are also many hairpins, by some called borers, chipped from chalcedony, agate, translucent flint, delicate and fragile, from 2 to 5 inches in length. A type of arrow-head is found at the site that is very peculiar and resembles in form some found in England, having long barbs, square at their points. Again are found long slender blades of the California type from 4 to 6 inches in length. The pipes, discoidal stones, ornaments, compare favorably with those from any other southern section. One peculiarity in regard to objects found at this village site is the absence of polished hatchets, that is, only a few are found. There are large numbers of flint and chert axes of similar form, chipped but never polished. Grooved axes are common, but not of the finest character, that is, no very large ones. Mortars, pitted stones and all and every implement and utensil used by the aborigines are present. Large ollas of the California type are found, made of soapstone. Very large spear-heads, 4 to 7 inches long, of flint, chert, and quartz are found in large quantities. Most of the objects are of quartz ; chert is next used, then jasper and flint of every color and shape. Many unique types

are found. Very diminutive arrow-points from 1¼ to 2 inches in length
are found of every kind of material.

I shall thoroughly examine, in the future, the shell heaps, and report
the results of my researches.

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CHEROKEE RIVER CULT. BY JAMES MOONEN, Bureau of American Eth-
        nology (read by Prof. McGee).

[To be published by Bureau of Ethnology.]

EXTENT OF INSTRUCTION IN ANTHROPOLOGY IN EUROPE AND THE
        UNITED STATES. BY GEORGE GRANT MACCURDY, Yale University,
        New Haven, Conn.

Regularly authorized instruction in anthropology dates from the second
half of the present century. Before passing the threshold of the next, it
might be well to have the benefit of any inspiration which may be drawn
from the progress of this new science as a branch of university discipline.

The time, the closing of a century, for such a review is, of itself,
opportune. Even if it were not so, occasion would not be wanting in the
independent movement in different countries looking toward the establish-
ment of chairs and lectureships of anthropology. Professor W J McGee's
efforts along that line in this country are noteworthy. Professor Wilhelm
Waldeyer in his inaugural address about a year ago as Rector of the
University of Berlin strongly emphasized the desirability of instituting
chairs of anthropology in the universities of the German Empire.[1]

The Anthropological Section of the British Association for the Advance-
ment of Science at the Bristol meeting, September, 1898, appointed a
Committee to ascertain "The present state of anthropological teaching
in the United Kingdom and elsewhere." Professor E. B. Taylor was
made Chairman of this Committee, and Mr. H. Ling Roth, Secretary.
Funds were voted for carrying on the investigation. The results of this
Committee's work are, no doubt, forthcoming in the report of the Dover
Meeting of the British Association which is to be held in September, 1899.

To go back half a century, Professor Serres held the Chair of Anatomy
at the Natural History Museum of Paris when it became the Chair of
Natural History of Man or *Anthropology*, as Serres himself called it in
announcing his course.

In 1867 Paul Broca opened a laboratory of anthropology in connection
with the *Société d'Anthropologie de Paris*, then already eight years old.
This laboratory became part of the *École pratique des Hautes Études*

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[1] Ueber Aufgaben und Stellung unserer Universitäten seit der Neugründung des
deutschen Reiches. Berlin. 1898. Druck von W. Büxenstein.

the next year (1868). As early as 1870 Broca had already established a regular course of lessons which was kept up until 1876, when it was merged in the newly-founded *École d'Anthropologie de Paris*. The latter was the first and remains the only school of its kind in the world.

Across the Channel, Sir William Flower had this to say in 1881 : "In not a single university or public institution throughout the three kingdoms is there any kind of systematic teaching, either of physical or of any other branch of anthropology, except so far as comparative philology may be considered as bearing upon the subject."[1]

In 1894 Sir William Flower could still say: "A professorship of Anthropology does not exist at present in the British Isles."[2] Instruction in some branches of anthropology were already being given, however, both at Oxford and Cambridge.

At Oxford, E. B. Tylor was made University Professor and Reader of Anthropology, December 31, 1898. Professor Tylor is also keeper of the University Museum. As he was the first Instructor in Anthropology (since 1883) in the British Isles, so is he the first Professor and the only one. .Arthur Thomson, University Professor of Human Anatomy gives instruction in physical anthropology, and Mr. Henry Balfour, Curator Pitt-Rivers Museum, lectures on : 'Arts of Mankind and Their Evolution.'

At Cambridge, Dr. Haddon, F.R.S., and Mr. W. H. L. Duckworth have, for some time, been recognized teachers of anthropology, and a lecturer on the subject has just been appointed. Alexander Macalister, Professor of Human Anatomy has for a number of years, found time to give instruction in physical anthropology.

Sir William Turner of Edinburgh (Professor of Human Anatomy) delivers a special course of lectures with practical demonstrations, in physical anthropology. A Museum of Anthropology was recently established at the University of Aberdeen, so that instruction in anthropology may, in all probability, be given there.

In Ireland, Dr. C. R. Browne, of Trinity College, Dublin, gives demonstrations in anthropometric methods. In addition to the work done in the Anthropometric Laboratory, every year, the instruments are taken to some selected district in Ireland and a systematic study of the inhabitants is made. The Royal Irish Academy makes yearly grants to the committee in charge of this work, the character of which may be ascertained from Dr. Browne's recent report on "The Ethnography of Clare Island and Inishturk, Co. Mayo."[3]

Germany has but one professorship of anthropology—that at Munich held by Johannes Ranke. To quote Professor Wilhelm Waldeyer who speaks especially for Munich and Berlin :

"Nur in München ist ein Professor ordin. für Anthropologie angestellt ; derselbe hat auch ein besonderes Institut und einen Assistenten, Hrn. *Dr.*

1 Presidential address to the Department of Anthropology, British Association, for the Advancement of Science (York meeting).

2 Presidential address to the Section of Anthropology, B. A. A. S. (Oxford meeting).

3 Proc. Roy. Irish Acad., 3d ser., Vol. V., No. 1, Dec., 1898.

Birkner. Sie wissen, dass *Johannes Ranke* der Professor ordin. ist.

" An den übrigen deutschen Universitäten werden zwar anthropologische Vorlesungen gehalten, aber wohl nur von Professores extraordinarii und Privat Docenten, ohne besonderen Lehrauftrag seitens der Regierung, rein als Privatsache, und es bestehen keine Institute für Anthropologie.

" Hier in Berlin lesen seit einigen Jahren :

" (1) Dr. *von Luschan*, Titular professor, über Physiche Anthropologie und über Ethnologie ; ferner gibt er im Völkermuseum (ganz unabhängig von der Universität) anthropologische und ethnographische Uebungskurse. (2) Professor Dr. *Wilhelm Krause*, Laboratoriumsvorstandt und der anatomischen Anstalt, liest über ' Rassenkunde ' und gibt Uegungen in ' anthropologischer Messungskunde.' (3) Dr. Seler, Geschichte und Alterthumskunde Mexico's. (4) Dr. *Huth*, Geschichte und Völkerkunde Siberiens.

" Wie es an den andern Universitäten ist, weiss ich nicht, abgesehen von dem, was ich vorhin gesagt habe."

Professor Ludwig, of Bonn, who occupies the Chair of Zoology and Comparative Anatomy, gives, in addition, a course in physical anthropology. Emil Schmidt (Prof. ordin. hon.) of the University of Leipzig, offers ' Anthropologie und Ethnologie " together with ' Anthropologische Uebungen.'

At Marburg i. H., P. Kretschner (Professor extraordin.) lectures on ' Indogermanische Völkerkunde und Urgeschichte Europas ' ; at Halle, Professor Kirchhoff, offers, among other courses, one in ' Anthropogeographie ' ; and at the Stuttgart Königl. Technische Hochschule, Professor Karl Benjamin Klunzinger gives instruction in anthropology and hygiene, in addition to zoology.

No French university offers a course in anthropology with the possible exception of Lyons where Ernest Chantre is Professor of Ethnology. This seems strange when we remember that the land of Buffon, Broca, de Quatrefages, and de Mortillet is looked upon as a pioneer in the anthropological sciences, and has trained a majority of all who are now teaching the subject. Channels of instruction have been found other than the universities—namely, the *École libre d'Anthropologie de Paris* and the *Museum d'Histoire Naturelle* at the *Jardin des Plantes*. The École d'Anthropologie offers nine courses by as many professors. They are as follows :

Matthias Duval ; *Anthropogénie, Embryologie.*

André Lefèvre ; *Ethnographie et Linguistique.*

Letourneau ; *Sociologie.*

Hervé ; *Ethnologie.*

Manouvrier ; *Anthropologie physiologique.*

Capitan ; *Anthropologie préhistorique.*

Laborde ; *Anthropoligie biologique.*

Mahoudeau ; *Anthropologic zoologique.*

Schrader ; *Géographie anthropologique.*

A monthly *Revue* is published by the professors. The Laboratory of

Anthropolgy which forms a part of the system called *École pratique des Hautes Études en Sorbonne* very naturally finds a home at the *École d'Anthropologie*.

The Chair of Anthropology at the *Museum d'Hist. Naturelle* is occupied by Prof. Hamy. His colleague is Dr. Verneau, who also offers courses in anthropology at the *École Coloniale* and the *Hôtel de Ville*.

The Universities of Italy make a good showing. Giuseppe Sergi is Professor of Anthropology at Rome, and Director of the Anthropological Cabinet, and Giustiniano Nicolucci fills a similar position at Naples, where Drs. Penta and Zuccarelli are Docents for Criminal Anthropology. The *Regio Istituto di Studi Superiori Practici e di Perfezionamento*, Florence, has the distinction of a professor of anthropology in *Paolo Mantegazza*. In three other Italian Universities the subject is receiving attention. Professor Severi is the authorized Docent for 'Legal' Anthropology at Genoa; Professor Lombroso, for Criminal Anthropology at Turin; and Tito Vignolo for Anthropology and Comparative Psychology at Milan.

Spain and Portugal have, each, one chair of anthropology—at Madrid and Coimbra, respectively. At Madrid, Manuel Antón y Ferrándiz is Professor *Cathedratico* of Anthropology and the Natural History of Man; at Coimbra, Bernardino Luis Machado Guimarães is Professor *Cathedratico* of Anthropology and Palaeontology.

Dr. Rudolph Martin, sometime instructor in Anthropology at the University of Zurich, Switzerland, has just been promoted to an assistant professorship; Dr. Martin is also Docent for Anthropology in the Polytechnic School of Zurich.

Hungarian, German and Bohemian Universities all are contributors to anthropology in the Empire of Austria-Hungary:

Aurel Török, Professor ordin.; Anthropology and Ethnology, and Director of the Anthropological Museum, Budapest; Lubor Niederle, Professor ordin., Anthropology and Prehistoric Archaeology, Prague; and Moriz Hoernes, Professor extraordin., Prehistoric Archaeology, Vienna. At the latter University, Dr. Michael Haberlandt is Docent in General Ethnography, and Dr. Phillip Paulitschke's lectures are ethnographical although his title is Docent in Geography.

A chair of geography and anthropology was recently created at the Imperial Academy of Sciences, St. Petersburg; D. N. Anutchin is the occupant. At the University, E. J. Petri is Professor of Geography and Ethnography and Director of the University Geograghical Anthropological Cabinet.

In Moscow, D. N. Anutchin is Professor of Geography and Ethnography and Director of the University Anthropological Museum; and Dr. N. N. Charuzin is Docent for Ethnography.

In Holland, there is no professorship of anthropology, so far as appears. J. J. M. de Groot is Professor of Ethnography at Leyden; Dr. G. J. Steinmetz is Instructor in Ethnology at Utrecht; and Professor G. Jelgersma of Amsterdam lectures on Criminal Anthropology.

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Monsieur E. Houzé is Professor of Anthropology at the *Université libre de Bruxelles*, Belgium. The course given by Professor Houzé was inaugurated in 1884. At the new University, Brussels, Professor G. Delbastee gives lectures on Criminal Anthropology.

For Scandinavia, there is a chair of northern archaeology at the University of Christiania occupied by Professor O. Rygh. In the same Faculty, Yngvar Nielsen is Professor of Geography and Ethnography and Director of the University Museum of Ethnography.

The University of Athens possesses an anthropological museum ; Dr. K. Stephanos, the Curator, may possibly give some instruction in the subject.

Mention has already been made of the movement in the United States to give anthropology more general recognition as a branch of university discipline. It has already taken its place in the curriculum of a number of our leading institutions.

In the Peabody Museum of American Archaeology and Ethnology at Cambridge, Harvard University has a most suitable habitation for a department of anthropology—extensive collections, laboratories, special library, lecture rooms, all combined under one roof and management, with its own special faculty, endowments, fellowships and scholarships. Frederick Ward Putnam, Curator of the Museum and Professor of American Archaeology and Ethnology, Dr. Frank Russell, Instructor in Anthropology, and Roland B. Dixon, Assistant in Anthropology, offer a number of courses, both general and special. An anthropological club holding semimonthly meetings testifies to the lively interest in the subject at Harvard.

Only a few months ago a professorship of anthropology was created in Columbia University, New York, and Dr. Franz Boas, for several years Lecturer in Anthropology, was promoted to the Chair. The work of Professor Boas is done in part at the American Museum of Natural History and in part at the Psychological Laboratory of the University, where Dr. Livingston Farrand (Instructor in Psychology) gives courses in ethnology, one of them being half of a general introductory course in anthropology by Drs. Boas and Farrand.

At the University of Chicago there is a provisional union of sociology and anthropology in a single department. " The differentiation of an independent department of anthropology and ethnology is anticipated." Dr. Frederick Starr is Associate Professor of Anthropology and Curator of the Anthropological Section of Walker Museum.

At New Haven, Yale University has for several years had the benefit of a course in general anthropology based on Ranke's " Der Mensch." For this course we are indebted to William G. Sumner, Professor of Political and Social Science. Prof. Sumner's generous impulses and admirable fitness, equal to his sense of the University's need, has led him to assume, willingly, extra labor and responsibility. To such men, many a university has been indebted for the growth and present riches of its curriculum, and, many a new science, for its separate and vital existence.

Dr. E. Hershey Sneath, Professor of Philosophy, gives a course entitled ' Philosophical Anthropology,' based on Lotze's Microcosmus.

The appointment of George Grant MacCurdy as Instructor in Prehistoric Anthropology at Yale dates from May, 1898. His courses are given at the University Museum, where a Laboratory of Physical Anthropology is being established, and where anthropological collections are being arranged both for students and for the public.

At Clark University, Worcester, A. F. Chamberlain is Lecturer in Anthropology. Assistant Professor W. Z. Ripley (Sociology and Economics, Massachusetts Institute of Technology, Boston) gives a ' course of one term' in Anthropology at the Institute yearly ; and at Columbia University (New York) in the School of Political Science, a course of one term entitled now Racial Demography, being a study of the population anthropologically of Europe and the United States. It was formerly called anthropology, but the title has been changed this year as given.

At the National Capital, some of the universities are making use of the anthropologists connected with the United States National Museum. Thomas Wilson, Curator of the Division of Prehistoric Anthropology, lectures at the National University, and Otis T. Mason is Lecturer in Anthropology at the Columbian University.

M. M. Curtis, Professor of Philosophy, Western Reserve University, Cleveland, gives a course of lectures on the history and the main problems and bearings of anthropology, and A. S. Packard, Professor of Zoology and Geology, performs a like service for Brown University, Providence. During the month of March, 1899, Professor W J McGee, Ethnologist in charge of the Bureau of American Ethnology, Washington, D. C., gave, at the State University of Iowa, a course of eleven lectures in general anthropology to large audiences. Such a beginning argues well for the future growth and development of a recognized branch of instruction.

Instruction in anthropology at the Ohio State University may be said to have a beginning in the work being done by Mr. W. C. Mills, Curator of the Ohio Archaeological-Historical Society.

In the death of Professor Daniel G. Brinton both the University of Pennsylvania and the Philadelphia Academy of Natural Sciences have lost a valued teacher of the anthropological sciences. No one has yet been appointed to take his place.

In order to reduce the above information concerning extent of instruction in anthropology to a more compact form, use is made of the following table :

Countries.	Institutions.	Professors.	Assistant professors.	Instructors.	Total teaching force.	Faculties.
British Isles	4	1	0	8	9	Natural Science.
Germany	7	1	2	8	11	Philosophical.
France	4	11	0	1	12	Philosophical or Faculté de Lettres.
Italy	6	3	0	5	8	Philosophical ; Nat. Sci.; Med.
Spain	1	1	0	0	1	Science.
Portugal.........	1	1	0	0	1	Philosophical.
Switzerland......	2	0	1	1	1	Natural Science.
Austria-Hungary	3	2	1	1	4	Philosophical.
Russia	3	1	0	3	3	Natural Science.
Holland.........	3	0	0	3	3	Various.
Belgium	2	1	0	1	2	Medical.
Scandinavia	1	0	0	2	2	Philosophical.
United States....	11	1	1	15	17	Various.
	48	23	5	48	75	

Of the forty-eight institutions in the thirteen countries giving a place to anthropology in their curricula, eleven are located in the United States; and of the total teaching force of seventy-five, our own country is credited with seventeen. But in the matter of professorships, the United States suffers by comparison, being allowed only one out of twenty-three by the strict terms of the title—that at Columbia held by Dr. Boas.

The above table is intended to serve more as a comparison of figures than of forces. To know precisely what is being done for the science in the several countries, one would have to take account of anthropological publications, museums, societies and clubs, as well as of sections of general scientific associations and academies of sciences. Such a compilation is beyond the scope of the present article.

So much for the extent[1] of instruction in anthropology as the century closes. The importance of the subject as a branch of university discipline, its terminology and the faculty to which it should belong, have all been touched upon by such authorities as Daniel G. Brinton,[2] of Philadel-

[1] Corrections of, and additions to, the record are respectfully solicited. The writer is especially indebted to Monsieur le Ministre de l'Instruction publique et des Beaux-Arts, France; and Professors Wilhelm Waldeyer, Rector of the University of Berlin; Alexander Macalister, Cambridge, England ; E. Houzé, Brussels ; Moriz Hoernes, Vienna ; W J McGee, Washington, D. C.; W. Z. Ripley, Boston ; the Hon. W. T. Harris, U. S. Commissioner of Education ; and his Excellency the Royal Prussian Kultusminister.

[2] Anthropology as a Science and as a Branch of University Education, Phila., 1892.

phia, Friedrich Müller,[1] of Vienna, Ruldolph Martin[2], of Zurich, and Geo. A. Dorsey,[3] of Chieago.

Professor Brinton made a "brief presentation of the claims of anthropology for a recognized place in institutions of the higher education in the United States" and asked for "the creation in the United States of the opportunity of studying this highest of the sciences in a manner befitting its importance." His classification and nomenclature, and his general scheme for instruction in this science acted as a stimulus to discussion on two continents.

Brinton's principal subdivisions are :

    I. Somatology—Physical and Experimental Anthropology.

    II. Ethnology—Historic and Analytic Anthropology.

    III. Ethnography—Geographic and Descriptive Anthropology.

    IV. Archaeology—Prehistoric and Reconstructive Anthropology.

Professor Müller does not see the need of separating the Geographical Ethnos from the Historic Ethnos, and, therefore, makes three divisions with a professorship for each :

    I. Physical Anthropology.

    II. Ethnography and Ethnology.

    III. Prehistoric Anthropology. The first he would place with the medical faculty, the other two, with the so-called philosophical faculty of the German Universities. When the three professors cannot be had—an anatomist for somatology, an ethnologist and linguist for ethnology and ethnography, and a geologist and archaeologist for the prehistoric—then Müller would suggest a double division : (1) Physical and Prehistoric Anthropology and (2) Ethnology and Linguistics. This, however, would divide the professorship of Physical and Prehistoric anthropology between two faculties, giving half to the medical faculty and half to the philosophical.

Professor Martin, on the other hand, argues that "die ganze Anthropologie in der naturwissenschaftlichen Abteilung der philosophischen Fakultät ihren natürlichen Platz hat." This seems to be the more logical arrangement and the one adopted by practically every university professing to give instruction in the subject as shown in the table above.

The difficulties of placing anthropology with this faculty or that, are themselves evidence of the fundamental character of the science. A branch of instruction that may be claimed by different faculties, and, at the same time, not adequately represented in any, might justly claim title to a faculty of its own.

Anthropology has matured late ; has been waiting for the contributions other sciences in the course of their development were bound to make to

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[1] Die Vertretung der Anthropologisch-ethnologischen Wissenschaften an unsern Universitäten, Globus, Bd. 66, S. 245, 1894.

[2] Zur Frage von der Vertretung der Anthropologie an unsern Univeristäten, Globus, Bd. 66, S. 304, 1894.

[3] The Study of Anthropology in American Colleges. Archaeologist, Dec., 1894, Waterloo, Indiana.

her ; waiting till the prehistoric perspective came to supplement the historic, permitting man to take the same dispassionate view of self as of the rest of nature, till remote lands told their story of human variation and culture stages, and till the teachings of embryology and comparative anatomy were better understood. The development and succession of the sciences may be likened to the development and succession of the fauna of which man forms a part. As man is last and highest in the geological succession, so the science of man is the last and highest branch of human knowledge. It is to be hoped the overflow from the sciences contributing to anthropology may be properly conserved and so distributed as to find its way more generally to the channels of University instruction. Whether the channel chosen be an existing faculty or a new and separate one is not so important as the stream it has to carry ; and there is reason to believe that that stream is gaining in volume constantly.

After the foregoing article was in type, there came from his Excellency the Royal Prussian *Kultusminister* in answer to my request of May 16th last for information, a manuscript statement handed in to him, September 27, 1899, by Professor Wilhelm Waldeyer entitled "Bericht über das anthropologische Unterrichtswesen in Deutschland." From this the writer is able to supplement his own lists for Germany as follows :

Breslau, Dr. Partsch (Prof. ordin., Geography), "Völkerkunde Europas"; Göttingen, Dr. von Bürger (Prof. tit., Zoology), "Ursprung und Vorzeit des Menschen"; Heidelberg, Dr. H. Klaatsch (Prof. extraord., Anatomy), "Anthropologie"; Kiel, Dr. Krümmel (Prof. ordin., Geography), "Ausgewählte Kaptel der Anthropo-geographie"; Königsberg, Dr. Bezzenberger (Prof. ordin., Comp. Philology), "Urgeschichte Ostpreussens"; Strassburg, Dr. G. Schwalbe (Prof. ordin., Anatomy), "Anthropologie"; Tübingen, Dr. von Sigwart (Prof. ordin., Philosophy), "Philosophical Anthropology."

This increases the number of German universities giving instruction in anthropology by seven, but does not augment the number of professorships.

Dr. W. H. L. Duckworth, is the newly appointed University lecturer in physical anthropology at Cambridge.

Two of the professors mentioned above have recently died; *viz.*, Edward Petri, of St. Petersburg, and Oluf Rygh, of Christiania.

OBSERVATIONS ON AFTER-IMAGES AND CEREBRAL LIGHT. BY E. W. SCRIPTURE, Yale University, New Haven, Conn.

A report was made of several observations going to show that the forms of light seen in external darkness were to be considered as cerebral in origin rather than as due to chemical changes in the retina as usually sup-

posed. The figures are always single, not double, and are generally quite distinctly bounded. This could hardly be the case if chemical changes were going on in the two retinas; it is rather to be concluded that the figures arise in the higher brain centers most directly concerned with the sensations of sight.

When the eye is displaced by the fingers, external objects appear to move with every displacement. The cerebral figures do not move. After-images also do not move. A series of experiments with cerebral figures, after-images, Purkinje's figures, and an image of the yellow spot, singly and in various combinations with each other, showed that none of them moved with displacements of the eye. External objects appear to move because the optical image is moved over the retina, but movements of the physiological mechanism have no effect. Thus these experiments fail to give any definite answer concerning the cerebral or retinal origin of cerebral light and after-images.

Experiments were also made with binocular after-images. The eyes viewed two incandescent lamps separately through tubes. The filaments of the lamps were at right angles. The after-images obtained were very strongly marked. As the tops of the filaments did not coincide but tended to produce a crossed image, this part of the combined after-image (seen when the eyes were directed to a white wall) was subject specially to binocular strife; the two images were continually alternating with only one constant portion, namely, that where the two images naturally joined together. The other parts of the two images remained steadily seen with reasonable constancy. In the case of the cerebral figures there is none of the appearance and disappearance so familiar in binocular strife and there is none of the change in form from combinations and dissolutions. During long and careful watching of the figures I find that they arise with forms as definite as those of clouds, often with sharp edges, and that they change forms and pass to one side just as very rapid clouds might. There is not a trace of a combination of two images.

ECONOMY OF SLEEP. BY E. W. SCRIPTURE, Yale University, New Haven, Conn.

The human body possesses many means of storing up energy which may be used as occasion requires. One of these means is sleep. During sleep the active portions of the body diminish their activity while the assimilation of energy still goes on to a considerable degree. Thus, the nerve cells—which at the end of the day are somewhat shrunken and have altered nuclei—regain their original forms during the rest provided by sleep. If the sleep is interfered with, the process of restoration is more or less hindered and less energy is stored up.

Long observation of my own case and of others lead me to believe that

the following proposition is, in general, true : the amount of energy available during the day increases and decreases as the storage during sleep increases and decreases.

You have all noticed the relation in a general way. If for any cause—grief, excitement, or noises—your sleep is frequently cut short, you find that your general energy diminishes. You also know how refreshing sound healthy sleep is. Now I wish to push this principle further.

In the first place an increase in the time of sleep increases the storage of energy steadily but with diminished acceleration. The increase of energy as depending on hours of sleep would probably be represented by some such curve as shown in the figure.
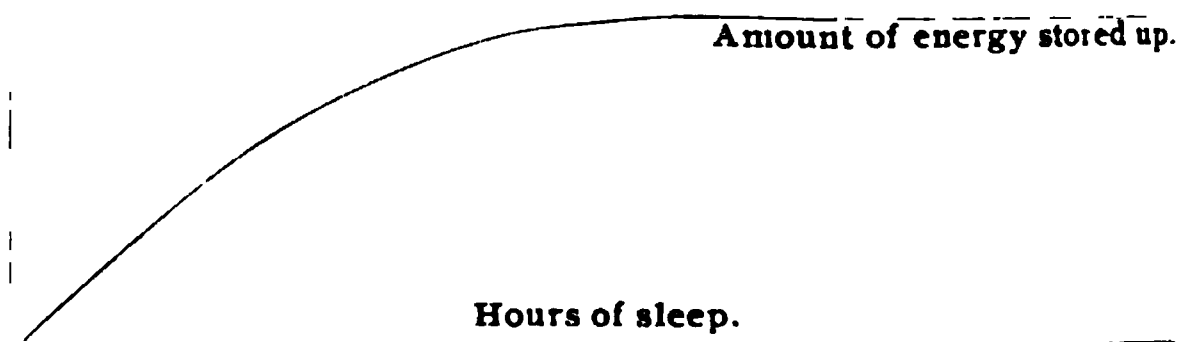
We do not keep this process going indefinitely because sooner or later something wakes us up, i. e., stops the sleep. It is evident, I think, that any means that can postpone the moment of waking, i. e., extend the time of sleep, will increase the amount of energy stored up for use during the day. Thus one of the principles in the economy of sleep is to postpone the moment of waking. A study of the methods of doing this would be a legitimate and highly valuable section of science.

What is waking? It might be briefly stated to be the passage from a state of less activity wherein the person is less responsive to the external world to a state of greater activity wherein he is more keenly responsive. During sleep we do not feel, hear, or see the events occurring around us. Yet all these events are sending sound waves that strike the ear light that strikes the eyes, heat and mechanical impulses that impress the skin. Moreover, these impulses send nerve currents to the brain, and undoubtedly are felt in the mind to a minute degree.

Why do we wake ? In our present civilization it is nearly always because some of the external impulses are sufficient to arouse us to activity. The amount of external impulse is not constant. In experiments of Kohlschütter and Michelson on sleep, little balls were dropped upon a board near a sleeper's head. The height from which the ball must be dropped, i. e., the amount of noise, depended upon various factors, such as the time during which the subject had already been asleep. Similar results would probably be found for other sensations. Such stimuli are always reaching us, and sooner or later we become rested enough to perceive them.

How shall we prolong sleep? Or how shall we postpone the moment of waking? If we wake because external stimuli affect us, we can postpone the moment of waking by diminishing the external stimuli.

Perhaps the first thing that wakes animals usually is the night-cold especially the cold just before dawn. In order to sleep, animals curl in groups, etc. Possibly one of the first uses of hides or other covering by primitive man was to enable him to sleep during the cold of the night. Few of us could sleep uncovered even in a moderately cool room. Another cause of waking is to be found in the sensations of the skin and bones. Thus even the animals provide themselves beds. Likewise we try to obtain quiet rooms with blinds or dark curtains to avoid sound and light. Can these methods be improved? If they can, we can gain more energy for use during the day.

In regard to cold I have no improvements to suggest; we can add bed covering of light weight until we are as warm as we like. The change of temperature during the night ought to be followed by an appropriate change in the bed covering or in the temperature of the room. There is apparently no means of doing this except in a heated room where there is an automatic regulator.

In regard to the skin and joint sensations the only improvement beyond a very fine hair mattress is the use of a small soft pillow for the heel. On account of the outward turn of the feet one heel of a person lying on his side must be raised above the toes that press on the bed or the leg must be twisted. The first position quickly produces an ache in the leg; the latter is accompanied by sensations that can wake the person at a given point of his storage of energy.

In regard to sound we are in a sad condition. Rattling wagons, trolley gongs, striking tower clocks, and the terrific bombardment of sound waves in the city, may be replaced by the screeching insects, chattering birds, and rustling trees of the country, but even in the most carefully selected sleeping room there is nearly always enough sound to waken the sleeper long before the two causes previously mentioned would act. Relief might be found in specially built rooms to which no sounds can penetrate; such sleeping rooms may be devised by the architect of the future but for us they are out of the question. The only other relief seems to lie in keeping the sound waves out of the ears. This may be done to a certain extent by pellets of moistened blotting-paper. The improvement in sleep that can be brought about by these pellets is very considerable. Still more sound can be kept out by antiphones of wax. [Demonstration of piece of special wax.] Light may be, to a great extent, avoided by black curtains or blinds. The avoidance may be pushed still further by a band of soft black cloth slipped around the head over the eyes.

The significance of these improvements in sleep lies in the gain in energy which the individual has ready for the struggle of life. Though the gain may be small, still it is the small steady profit that often makes the difference between success and failure.

Suppose that the average gain of 10 per cent. in time of sleep could be made and that this resulted in an average gain of 5 per cent. in energy. This gain might express itself in greater quickness or greater acuteness of thought. This would be a tremendous advantage to an individual, a community, or a race. It might express itself in greater courage, greater resistance to disease, etc.

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ADDRESS

BY

MARCUS BENJAMIN, Ph.D.,

VICE-PRESIDENT, AND CHAIRMAN OF SECTION I.

THE EARLY PRESIDENTS OF THE AMERICAN ASSOCIATION.

For a second time in its history the section on Social and Economic Science in the American Association for the Advancement of Science has chosen for its presiding officer one whose early training was that of a chemist. It had been my hope to present before you an address that should treat of certain phases of the development of industrial chemistry in the United States. The suggestion, however, made at the midwinter meeting in New York by Professor Putnam, that I prepare an account of the early history of the Association, appealed to me so strongly that I was very glad to yield to the wishes of the council, who promptly accepted the recommendation of our distinguished president, and, therefore, I have the honor of addressing you on The Early Presidents of the American Association.

The American Association for the Advancement of Science came into formal existence in the city of Philadelphia, on September 20, 1848. The prevalent **HISTORY** fondness for genealogical research affords us an excellent excuse for a brief discussion of its ancestry.[1]

[1] In *The Chautauquan*, vol. XIII. p. 727, September, 1891, there is a historical sketch of The American Association for the Advancement of Science, by the present author, which may be of some interest to the student of the history of American science.

For a century preceding the existence of our Association, Philadelphia had held foremost rank as a scientific center. It was in that city as early as 1743 that Benjamin Franklin—America's first great scientist—had made futile effort to form a society "of *virtuosi* or ingenious men residing in the different colonies to be called the American Philosophical Society."[1] That society, however, as is well known, did organize in 1769. and still survives, the oldest of scientific societies in the United States. An interesting evidence of the fact that Philadelphia was a mecca to scientific men is the statement that Priestley, on his arrival in New York in June, 1794, declined to give a course of lectures in that city, and proceeded at once to Philadelphia, where he received a complimentary address from the American Philosophical Society.[2]

In the early dawn of the new century came that wonderful development of science in New Haven, brought about by the influence of the elder Silliman, who, by the way, first studied chemistry in Philadelphia under James Woodhouse. In the year 1819, in the philosophical room of Yale College, there was organized the American Geological Society, of which, according to G. Brown Goode, our Association "is essentially a revival and continuation."[3] "Its members," says the same authority, "following European usage, appended to their names the symbols ' M.A.G.S.,' and among these were many distinguished men, for at that time almost every one who studied any other branch of science, cultivated geology also."[4] If we accept the American Geological Society as our ancestor, it gives the American Association rank as the fifth oldest scientific body in the United States.

As knowledge grew and education advanced, the desire for frequent intercourse among men of science increased more and more, and in the rooms of the Franklin Institute in Philadelphia on April 2, 1840, there was organized the Association of American Geologists. This society, which two years later became the Association of American Geologists and Natural-

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[1] The Origin of the National Scientific and Educational Institutions of the United States, by G. Brown Goode. Annual Report of the American Historical Association for the year 1889. Washington, 1890, p. 54.

[2] The Development of Science in New York City, by Marcus Benjamin. Memorial History of the City of New York. New York, vol. IV, p. 415.

[3] Goode, *op. cit.*, p. 112.

[4] Goode, *op. cit.*, p. 112.

ists, is officially recognized as our progenitor, and the record of the eight meetings is given in the preliminary pages of our annual volume of proceedings. Of the founders of that Association the venerable Martin H. Boyé still survives,[1] and in New York City, Oliver P. Hubbard, who served as its secretary in 1844, remains to us a living witness of the mighty events that have occurred in the golden era of science.

It will not be out of place, I am sure, to mention the influence of the National Institution for the Promotion of Science on the formation of our Association. It was that Institution, which in April, 1844, brought together in Washington city the first National Congress of scientific men—the first cosmopolitan assemblage of the kind which in any respect foreshadowed the great congresses of the American Association in later years.[2]

This Institution, so successful that it was perhaps the most powerful " agency in setting in operation the influences which led to the establishment of the Smithsonian Institution, the National Observatory, the National Museum, and the Department of Agriculture, and in later years, of the National Academy of Sciences,"[3] yet so unsuccessful that " the Smithsonian fund, which it aspired to control, was placed under other authority; the collections and manuscripts of the United States Exploring Expeditions were removed from its custody; the magnificent collection in natural history, ethnology, and geology, which had accumulated as a result of its wonderful activity and enthusiasm, soon became a burden and a source of danger,"[4] was abandoned by its founders and supporters, and finally in 1861 went out of existence by the termination of its charter.

Its remarkable history has been told by G. Brown Goode in a paper in which he showed its connection with the organization of our Association. In closing he said of the American Association :

[1] The *Scientific American*, vol. LXXIV, p. 430, for December 12, 1896, under the title of A Pioneer of Science, gives an interesting account of Martin H. Boyé with a portrait.

[2] The First National Scientific Congress (Washington, April, 1844) and its connection with the organization of the American Association. Report U. S. National Museum, 1897, p. —. (In press.)

[3] *Idem*, p. —. (In press)

[4] *Idem*, p. —. (In press.)

The new society was born, and it is significant that the name first adopted was as nearly as possible a combination of the names of the two societies. The one contributed the first half of the name "The American Association," the other the second half—"for the Promotion of Science." The word advancement in place of promotion was substituted afterwards.[1]

The history of the Association is a task that must be left for other, more competent, members to present to you. To me has been assigned the duty of briefly reviewing the career of that brilliant galaxy of men who have been chosen by you to preside over the meetings of the American Association for the Advancement of Science.

William C Redfield, "who was the first to suggest the idea of the American Association in its present comprehensive plan, and the first to preside over its deliberations,"[2] **REDFIELD** was born in Middletown, Connecticut, on March 26, 1789. As a boy he received only the simplest rudiments of education, and at the age of fourteen was apprenticed to a saddler. At that time he evinced a remarkable fondness for books, and, we are told that "he was denied even a lamp for reading by night much of the time during his apprenticeship, and could command no better light than that of a common wood fire in the chimney corner."[3] Through the interest of Dr. William Tully, a learned and distinguished physician of Cromwell, Connecticut, he was afforded the privilege of that good doctor's library and chose Sir Humphrey Davy's Elements of Chemistry with which to occupy his leisure moments. In returning the book he surprised its owner by showing a thorough acquaintance with its contents, and in particular with the doctrine of chemical equivalents, which, he said, he had then met with for the first time.

On the completion of his apprenticeship, early in 1810, he made a long journey on foot to Ohio, passing through New York and northern Ohio, when "the sites of Rochester and Cleveland were dark and gloomy forests, and Buffalo was a mere hamlet."[4] He returned to New England in the follow-

[1] Goode : First National Scientific Congress.

[2] Address on the Scientific Life and Labors of William C Redfield, A.M., first president of the American Association for the Advancement of Science, delivered before the Association at their annual meeting in Montreal, August 14, 1857. By Denison Olmsted. with an engraved portrait on steel. Cambridge, 1858, p. 3.

[3] *Idem*, p. 5.

[4] *Idem*, p. 7.

WILLIAM C. REDFIELD.

ing spring, choosing on this occasion a more southerly route through parts of Virginia, Maryland, and Pennsylvania. This journey deserves special mention because it was from the observations made by him then that he was able later to advocate with such remarkable power the great superiority of railroads to canals, and also the plan of a system of railroads connecting the waters of the Hudson with those of the Mississippi. In a pamphlet, which he issued in 1829, he startled the community by the boldness of his project. He says, referring to the territory east of the Mississippi :

This great plateau will indeed one day be intersected by thousands of miles of railroad communications; and so rapid will be the increase of its population and resources, that many persons now living will probably see most or all of this accomplished.[1]

To the scientific world Redfield, however, is best known by his development of the law of storms. Essentially his theory was that a storm was a progressive whirlwind. For years he kept his theory to himself, and it was not till accumulative evidence established in his own mind the correctness of his convictions that he gave to the world through the *American Journal of Science* his valuable series of papers on that science which we now dignify by the name of meteorology.

Through the long years of his life Mr. Redfield was actively engaged in business, having established a line of tow barges between New York City and Albany, and it was only such time as could be spared from more important pursuits that he devoted to the higher cause of science. The fossils, the ripple marks, and the rain-drops in the sandstones of Connecticut and New Jersey interested him, and the papers which he read before this Association towards the close of his career pertained to his studies in geology. He died in New York City on July 12, 1857.

The selection of Joseph Henry in 1846 to be the guiding hand of the then newly established Smithsonian Institution made him perhaps the most conspicuous rep-   **HENRY**
resentative of American science of his time. Henry
was born in Albany just a century ago, and there he grew up and was educated. As a student, as a teacher, and as a professor he

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[1] Route of a Great Western Railway, 1829.

was connected with the Albany Academy, and in that institution he carried on those researches in electricity which made the electromagnetic telegraph of Morse possible. In other words, Henry was the first to construct and use an electromagnetic acoustic telegraph of a type similar to that which is at present more generally employed than any other form. The code of signals now in general use had not at that time been invented.[1] In referring to his researches Sir David Brewster says: "On the shoulders of young Henry has fallen the mantle of Franklin."[2] In 1832 he accepted a call to the chair of Natural Philosophy in Princeton, and for fourteen years led the peaceful life of a college professor in a rural university town.

Then came the call to Washington, and dubious as to the future, he said: "If I go I shall probably exchange permanent fame for transient reputation."[3] The path of duty was clearly defined, and yielding to the solicitation of his associates, such as Bache, Hare, and Silliman, he accepted the appointment of secretary of the Smithsonian Institution. Of his career in Washington a contemporary says:

Called to administer the Smithsonian trust, his conscientious devotion gave it from the first the direction designed by the testator. His aim was to originate and disseminate. He scattered the seed broadcast, not through whim or favoritism, but on a matured plan. His place required a love of science, along with a talent for organization. He brought these to bear upon the origination of knowledge, and by his scientific sympathy and ready recognition of others of his guild he commanded honest homage and became the director, helper, and umpire in scientific disputation. Did the War Department require his aid in meteorology? He gave the plan of weather signals. Did the Census Bureau ask his help? He planned the remarkable atlas as to rain-falls and temperature. Did the Coast Survey require scientific suggestion, or the Centennial Commissioners his judgment, or the new library and the "School of Art" a friend and adviser, or the Light House Board laws of sound for fogs, and cheaper and better illumination? He freely gave what ·was gladly welcomed. His Institution gave Agassiz opportunity to study fishes, Baird, birds, and all students encouragement to investigate our American archaeology and ethnology, as well as our fauna and flora.[4]

Those who are willing to know more of Henry's great work

[1] Sketch of Joseph Henry, by G. Brown Goode, in The History of the First Half Century of the Smithsonian Institution. Washington, 1897, p. 134.

[2] *Idem*, p. 122.

[3] A Memorial of Joseph Henry, with an engraved portrait on steel. Washington, 1880, p. 276. Discourse of Wm. B. Taylor.

[4] *Idem*, p. 103. Address of Hon. S. S. Cox.

need only consult The Memorial Volume,[1] published by the Smithsonian Institution shortly after his death. I add the last sentences of Goode's sketch of him, which was published in the History of the First Half Century of the Smithsonian Institution:

What Franklin was to the last century, Henry is to this, and as the years go by his fame is growing brighter. The memorial service in his honor, held in 1878, in the hall of the United States House of Representatives, was a national event. In 1883 his monument in bronze, by the greatest of American sculptors, was erected by Congress in the Smithsonian Park. The bestowal of his name upon the unit of induction in 1893 was an indication of his foreign appreciation, while, as a still nobler tribute to his fame, his statue has been placed under the great rotunda of the National Library, the science of the world and of all time being symbolized by these two great men, Newton and Henry.[2]

Beginning with 1850 the Association inaugurated the custom of holding a meeting in the spring of the year as well as one in the late summer. These earlier gatherings were held in the cities of the south and west, and the first of them, in March, 1850, was convened in Charleston, South Carolina, then a city of much scientific activity. Over this meeting Alexander Dallas Bache was chosen to preside.

Birth, education, and association combined to qualify Bache in an unusual degree for the many important duties to which he was called. He was the son of Richard Bache, one of the eight children of Sarah, the only daughter of Benjamin Franklin, and was born in Philadelphia in 1806. **BACHE** He was educated at the United States Military Academy in West Point and graduated at the head of the class of 1825 (of which he was the youngest member) with the unusual distinction of completing that rigid course of four years without receiving a single demerit. An appointment in the Corps of Engineers followed, and after serving a year as assistant professor of engineering at West Point, he was assigned to duty under Colonel Joseph G. Totten, in Newport, Rhode Island.

In 1829 he resigned from the army to accept the chair of natural philosophy and chemistry in the University of Pennsylvania, in Philadelphia, where he remained until 1843, lead-

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[1] A Memorial of Joseph Henry. Washington, 1880.

[2] The Smithsonian Institution, 1846-1896. The History of its First Half Century. Washington, 1897, p. 156.

ing a life of great activity, for he was a guiding influence in nearly every scientific movement in the city of his birth. He was appointed chairman of one of the most important of the committees of the Franklin Institute, and was chosen as the expounder of the principles of the institute at its public exhibitions. He was an active member of the American Philosophical Society, and in his private observatory began that series of magnetic observations with which his name is so honorably connected.

His services in establishing the Girard College, of which he was the first president, and his development of the public school system of Philadelphia while filling the offices of principal of the high school and that of superintendent of the public schools, are best described in the statement that "the result of his labors in regard to the schools was the establishment of the best system of combined free education which had, at that time, been adopted in this country. It has since generally been regarded as a model, and has been introduced as such in different cities of the Union."[1]

Bache's great work, however, was in connection with the United States Coast Survey, to the superintendency of which he was called in 1843, and of his relation to that work I again quote from his biographer:

When Professor Bache took charge of the survey, it was still almost in its incipient stage, subjected to misapprehension, assailed by unjust prejudice, and liable, during any session of Congress, to be suspended or abolished. When he died, it had conquered prejudice, silenced opposition, and become established on a firm foundation as one of the permanent bureaus of the executive government. . . . . He divided the whole coast line into sections, and organized, under separate parties, the essential operations of the survey simultaneously in each. He commenced the exploration of the Gulf Stream, and at the same time projected a series of observations on the tides, on the magnetism of the earth, and the direction of the winds at different seasons of the year. He also instituted a succession of researches in regard to the bottom of the ocean within soundings, and the forms of animal life which are found there, thus offering new and unexpected indications to the navigator. He pressed into service, for the determination of the longitude, the electric telegraph; for the ready reproduction of charts, photography; and for multiplying copper-plate engravings, the new art of electrotyping. In planning and directing the execution of these varied improvements, which exacted so

[1] Eulogy on Professor Alexander Dallas Bache, late Superintendent of the U. S. Coast Survey, by Joseph Henry. Smithsonian Report for 1870, p. 98.

much comprehensiveness in design and minuteness in detail, Professor Bache was entirely successful.[1]

In Washington, as in Philadelphia, he was foremost in every movement, public or private, that tended toward the advancement of science. Besides being *ex-officio* superintendent of Weights and Measures, he was a member of the Lighthouse Board, and a regent of the Smithsonian Institution from its inception till his death. Nor can I omit mention of the fact that he was a vice-president of the United States Sanitary Commission, and first president of the National Academy of Sciences. Professor Bache presided over the Charleston meeting in 1850, and also over the New Haven meeting in August, 1851, and over the Cincinnati meeting in May of the same year.

It is difficult at this time to determine when the unwritten law of the Association that a representative of the natural sciences should be chosen to succeed a representative of the physical sciences in the presidential chair came into existence, but with the election of Louis Agassiz, in 1851, as the successor of Bache, the principle was clearly indicated.

With the possible exception of the elder Silliman the influence of Louis Agassiz on the development of science in our country has been greater than that of any other single man. The extraordinary personal quali- **AGASSIZ** ties of character as well as the talents and attainments of this great naturalist make any attempt of a brief sketch of his career almost impossible.[2]

The son of a Protestant clergyman, he was born in Switzerland, in 1807, and his early academic education was obtained in Bienne, Lausanne, and Zurich, whence he passed to the great German universities of Heidelberg, Munich, and Erlangen. Even in those days he was a leader. In Munich he was the presiding officer of the Little Academy, the members of which have since enrolled their names high on the tablets of fame. At the age of twenty-one, even before the doctor's degree had been conferred upon him, young Agassiz

[1] Henry's Eulogy on Professor Alexander Dallas Bache, pp. 100, 101.

[2] See Louis Agassiz. His Life and Correspondence, edited by Elizabeth Cary Agassiz, with portraits on steel, 2 vols. Boston, 1885.

had secured "a place among the best naturalists of the day"[1] by his work on the fishes of Brazil.

Delightful years in Vienna and Paris followed during which his dissipations were confined to the pleasures of association with the most distinguished men of his time, especially in Paris, where Humboldt was a conspicuous leader, and became his patron. Then, in 1832, he settled in Neuchatel as professor of natural history in the small college of that ever-charming little city. Students came to him ; and among his associates of that time were Guyot and Pourtales, whom even the ocean could not separate from him. His Recherches sur les Poissons fossiles in five quarto volumes, and his Etudes sur les Glaciers, were given to the world during his residence in Neuchatel. The former is perhaps his most important contribution to natural science, and the latter a pioneer work in glacialogy.

In 1846 an invitation to deliver a course of lectures before the Lowell Institute in Boston was obtained for him through the interest of his friend, Sir Charles Lyell, and he agreed with Mr. John A. Lowell to give a course of Lectures on the Plan of the Creation, especially in the Animal Kingdom. He arrived in Boston in October, and in December delivered his first lecture. "He carried his audience captive."[2] From that time the well-worn "Veni, Vidi, Vici" tells the story of his career in the new world. Enthusiastic audiences greeted him in New York, Philadelphia, Charleston, and elsewhere, and yielding to the irresistible opportunities offered to him, he severed the ties that bound him to the land of his birth and accepted the chair of zoology and geology in the Lawrence Scientific School.

Guyot, his friend from boyhood, in speaking of the immense power he exerted in this country in spreading the taste for natural science and elevating the standard, says :

How many leading students of nature are found to call themselves his pupils, and gratefully acknowledge their great indebtedness to his judicious training. How many who now occupy scientific chairs in our pub-

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[1] Biographical Memoirs of the National Academy of Sciences. Washington, 1886. Vol. ii, p. 49. Louis Agassiz, by Arnold Guyot.

[2] Life and Correspondence, vol. ii, p. 496.

lic institutions multiply his influence by inculcating his methods, thus rendering future success sure. [1]

No better evidence of his success as a teacher is needed than that of the mere mention of his famous students. In addition to his son, Alexander, the names of Bickmore, Brooks, Clark, Fewkes, Hartt, Hyatt, Lyman, Morse, Niles, Packard, Putnam, Scudder, Shaler, Stimpson, Verrill, and Wilder, come readily to mind.

In this connection I want to quote from a letter of one of his students[2] who wrote me concerning his teaching as follows :

The ideal of a young scientific student, and of every great teacher, is a devotion to scientific research for its own sake. Agassiz had that ideal extraordinarily developed, and on that account the student was drawn to him and felt in a corresponding degree a great influence on his life. Agassiz made many and important contributions to science, but the greatest of all was a life which embodied the ideal that scientific research is an unselfish study of truth for truth's sake. Every student who was brought in contact with Agassiz recognized this ideal, and was profoundly influenced by it.

The Museum of Comparative Zoology in Cambridge, is his most conspicuous monument, but his influence, more powerful than bricks or mortar, will live forever.

A bowlder from the glacier of the Aar marks his last resting place in Mount Auburn, and so "the land of his birth and the land of his adoption are united in his grave."[3]

The policy of holding two meetings a year was soon found to be unsatisfactory, and it was abandoned after the Charleston meeting in 1851. In consequence no spring gathering was held in 1852, and also no summer meeting was held during that year. It was not until July, 1853, that the Association again met, and then it was convened in Cleveland under the presidency of Benjamin Peirce.[4]

This distinguished mathematician, one of the greatest this country has ever known, was born in Salem, Massachusetts, in 1810. His father, whose name the son inherited, is best remembered as the historian and librarian of Harvard. In Cambridge the boy grew to manhood, and was graduated at Har-

[1] Memoir by Guyot, p. 71.
[2] Dr. J. Walter Fewkes.
[3] Life and Correspondence, vol. 11, p. 783.
[4] See Benjamin Peirce. A Memorial Collection, by Moses King. Cambridge, 1881. p. 18, with an engraved portrait on wood.

vard in 1829 in the class that Oliver Wendell Holmes has so beautifully immortalized in one of his charming poems.

While in college he became a pupil of Nathaniel Bowditch, "who made the prediction that young Peirce would become one of the leading mathematicians of this century."[1] After graduating he began his career as a teacher, and in **PEIRCE** 1831 returned to his *alma mater* as tutor in mathematics, becoming eleven years later Perkins professor of mathematics and astronomy, which chair he held until his death, "when he had been connected with the university for a longer time than any other person, except Henry Flynt, of the class of 1693."[2]

His election to the presidency of our Association was probably a result of his connection with the United States Coast Survey, as in 1852 he had been assigned to the charge of the longitude determinations in that service. The successful prosecution of that work, in which he was associated with some of our most distinguished members, indicated him as the natural successor to the superintendency of the survey itself on the death of Bache, in 1867.

The paramount events of the civil war had, to a large extent, interfered with the regular work of the survey, but under Peirce it was actively resumed. The plans laid down by his predecessor were taken up and the survey extended to a great geodetic system, stretching from ocean to ocean, thus laying the foundations for a general map of the country that should be entirely independent of detached local surveys. With this object the great diagonal arc was extended from the vicinity of Washington to the southward and westward along the Blue Ridge, eventually reaching the Gulf of Mexico near Mobile. He also planned the important work of measuring the arc of the parallel of 39 degrees to join the Atlantic and Pacific systems of triangulation; and for determining geographical positions in states where geological or geographical surveys were in progress.

Only an astronomer can follow the mathematical intricacies of Peirce's remarkable announcement concerning the discovery of the planet Neptune.

[1] Memorial Collection. p. 18.

[2] Cyclopaedia of American Biography, vol. VI, p. 701. New York, 1888. Article on Benjamin Peirce written by myself.

Joseph Henry

A. D. Bache

L. Agassiz

Benjamin Peirce

This planet [says President Hill] was discovered in September, 1846, in consequence of the request of Leverrier to Galle that he should search the zodiac in the neighborhood of longitude 325° for a theoretical cause of certain perturbations of Uranus. But Peirce showed that the discovery was a happy accident; not that Leverrier's calculations had not been exact, and wonderfully laborious, and deserving of the highest honor, but because there were, in fact, two very different solutions of the perturbations of Uranus possible : Leverrier had correctly calculated one, but the actual planet in the sky solved the other ; and the actual planet and Leverrier's ideal one lay in the same direction from the earth only in 1846. Peirce's labors upon this problem, while showing him to be the peer of any astronomer, were in no way directed against Leverrier's fame as a mathematician : on the contrary, he testified in the strongest manner that he had examined and verified Leverrier's labors sufficiently to establish their marvellous accuracy and minuteness, as well as their herculean amount.[1]

His greatest contribution to astronomy, however, was in connection with the rings of Saturn. He demonstrated that the rings, if fluid, could not be sustained by the planet, that satellites could not sustain a solid ring, but that sufficiently large and numerous satellites could sustain a fluid ring, and that the actual satellites of Saturn were sufficient for that purpose.

Peirce was a teacher, and his teaching is referred to by one of his students as " the most stimulating intellectual influence I ever encountered."[2] As an executive officer in charge of the coast survey, and also of the *American Ephemeris*, it is said that :

The reports of that Survey and the tables of the *Ephemeris* have rapidly raised the scientific reputation of America, which, in 1843, stood in astronomy among the lowest of civilized nations, and is now among the highest — a change which was by no means ungrateful to Peirce's strongly patriotic feeling, and which he could not but know was as much due to himself as to any other person.[3]

As a mathematician it was said at the time of his death that "the late Professor Peirce's merits will rank with the marvellous achievements of Bernoulli, Euler, and Laplace."[4]

President Hill closes his sketch of Peirce with the following words :

[1] Thomas Hill in The Memorial Collection, p. 8.
[2] Thomas Wentworth Higginson, in The Memorial Collection, p. 31.
[3] *The Nation*, New York, October 14, 1880.
[4] *Boston Daily Advertiser*, October 7, 1880.

While Professor Peirce has the tenacity of grasp, and power of endurance, which enable him to make the most intricate and tedious numerical computations, he is still more distinguished by intensity and fervor of action in every part of his nature, an enthusiasm for whatever is noble and beautiful in the world or in art, in fiction or real life; an exalted moral strength and purity; a glowing imagination which soars into the seventh heavens; an insight and a keenness of external observations which makes the atom as grand to him as a planet; a depth of reverence which exalts him while he abases himself.[1]

I prefer the stanzas of Holmes' Memorial poem, beginning with:

> To him the wandering stars revealed
> The secrets in their cradle sealed;
> The far-off, frozen sphere that swings
> Through ether, zoned with lucid rings;
>
> The orb that rolls in dim eclipse,
> Wide wheeling round its long ellipse,—
> His name Urania writes with these,
> And stamps it on her Pleiades.[2]

It was at the Toronto meeting just ten years ago that the Association was honored by the presence of its then oldest living past president in the person of James Dwight Dana, who in 1854, presided over the meeting held in Washington.

Dana was born in Utica, New York, in February, 1813, and as a boy showed a taste for natural science, making frequent excursions after minerals with his school companions.

DANA Attracted by the name of the elder Silliman, then at the height of his powers and reputation, he went to New Haven and entered Yale. As an undergraduate it is said that "he made much progress in science, especially in his favorite study of mineralogy."[3]

The influence of the master was irresistible, and he decided to devote himself to science, and, as if to confirm his decision, an opportunity presented itself even before he had graduated, for in 1833 he accepted an appointment as instructor in mathematics in the United States Navy. For more than a year he cruised in European waters, chiefly on the Mediterranean, de-

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[1] The Memorial Collection, p. 11.

[2] *Atlantic Monthly.*

[3] See James Dwight Dana, a biographical sketch, with a half-tone portrait, and bibliography, by E. S. Dana in the *American Journal of Science*, third series, vol. XLIX, p. 329, May, 1895.

voting his leisure to studies of the interesting features of geology and natural history that presented themselves.

He returned to New Haven in 1836, and became an assistant to Silliman. It was at this time, in May, 1837, that he published the first edition of his System of Mineralogy. Scarcely had that work been given to the public than he received an invitation to become the mineralogist and geologist of the United States Exploring Expedition about to visit the Southern and Pacific oceans under Captain Charles Wilkes. In August, 1838, the expedition started from Norfolk, Virginia, and reached New York on its return in June, 1842. For thirteen years thereafter Dana devoted himself to the study of the material that had been collected, and to the preparation of his reports, of which those on the Zoophytes, the Geology of the Pacific, and on the Crustacea were published.

Meanwhile he accepted the appointment to the Silliman chair of natural history and geology in Yale, but did not assume the active duties of the professorship until 1855. No greater scientist nor grander character has ever appeared in the history of American science than the elder Silliman, and I cannot refrain from introducing at this place his description of Dana's first lecture at Yale. He says:

Professor James D. Dana, my son-in-law, successor in the department of geology, began his course to-day in the geological room, the scene of my labors on the same subject. His lecture was very able and interesting; and, very unexpectedly to me, he introduced it with a notice of the rise and progress of geology in this country, and my action and influence in bringing it about were set forth in warm and elegant language. I was not indeed aware that he appreciated my efforts and attainments so highly. * * * It is a signal favor that I have lived to see my two extensive departments divided, and, without any influence of mine, my own son charged with chemistry, and my son-in-law with the mineralogy and geology; and I am still in health of body and mind to enjoy this happy result.'

From this auspicious beginning his active connection with Yale continued until it was interrupted in 1890 by a serious illness, after which, failing strength and advancing years made it impossible for him to resume his professorial duties, and in 1894 he was made professor emeritus.

¹ Life of Benjamin Silliman, by George P. Fisher, 2 vols., with engraved portrait on steel. New York, 1866. Vol. II, p. 237.

The year 1818 is conspicuous in the history of development of science in this country by the founding of the *American Journal of Science*. From its inception till his death the name of Benjamin Silliman appeared on its title-page as senior editor. In 1846 to that name was added those of the younger Silliman and Dana as associate editors. Of these three Dana was the survivor, and from 1875 till his death he was its senior editor.

In 1893, on the occasion of his eightieth birthday, a congratulatory letter from his scientific colleagues in New Haven made mention of his editorial career as follows:

The long series of volumes of this periodical are a noble monument of the extent and thoroughness of your labors as a naturalist.[1]

It is fortunate for American science that this journal has been handed down as a precious legacy to the grandson of its founder, Edward S. Dana, under whose able guidance, let us hope, that it may long continue.

Wherever mineralogy or geology is taught, the unsurpassed text-books on these subjects by Dana, hold easy supremacy. His System of Mineralogy, first published in 1837 as a volume of 580 pages, passed to a second edition in 1844, a third in 1850, a fourth in 1854, and a fifth in 1868, when it had increased to 827 pages. The later editions were prepared by his son. To these must be added four editions of his smaller Manual of Mineralogy, the last of which appeared in 1887, and was a duodecimo volume of 518 pages. Of his mineralogy, Powell says:

Thus he was the first to give us a system of mineralogy; but his work in this field did not end at that stage. He still pursued his investigations, collecting from many fields, and drawing from the collections of many others in many lands, until at last he developed a new system of mineralogy, placing the science on an enduring basis. This accomplishment alone was also worthy of a great man, and by it a new science was organized on a mathematical, chemical, and physical basis.[2]

The broader field of geology became his after his return from the exploring expedition, and he published his Manual of Geology in 1862. Of this work one of his colleagues says:

[1] *Science*, new series, vol. 1, p. 489. May 3, 1895.

[2] Memorial address on James Dwight Dana before the scientific societies of Washington, by John W. Powell. *Science*, new series, vol. III, February 7, 1896, p. 183.

The treatment of strata and fossils from a chronological point of view as historical geology is a characteristic feature of his manual. The growth and development of the earth, its continents and seas, and the progress in the organic life on its surface, were thus unified into a special department of geology, the history of the earth and of its inhabitants, which was by other authors dealt with as formational, stratigraphic, or paleontologic geology.

He prepared four editions of this work, the last of which appeared early in 1895, shortly before his death. As with his mineralogy he prepared an elementary text-book of geology, of which two editions were published. Concerning his valuable work on geology, Powell said :

So Dana's Geology is not only a text-book of geology, but it is the handbook for all national, state, and local geologists, and all students in the field. It is the universal book of reference in that department of science. Other text-books have been developed, but no other handbook for America. It is a vast repository of facts but, all arranged in such a manner as to constitute a geologic philosophy. It is on every worker's table and is carried in the kit of every field observer. It has thus become the standard to which all scientific research is referred, and on which geologic reports are modeled.[1]

Besides the foregoing, Dana was the author of Coral Reefs and Islands, which he enlarged and published later as Corals and Coral Islands; of The Geological Story Briefly Told ; The Characteristics of Volcanoes; and The Four Rocks of the New Haven Region.

In conclusion Powell says of him :

Dana as a zoologist was great, Dana as a mineralogist was greater, but Dana as a geologist was greatest, and Dana in all three was a philosopher; hence, Dana's great work is enduring.[2]

The ninth meeting of the American Association was held.in Providence, Rhode Island, and over that meeting John Torrey, " chief of American botanists,"[3] presided.

Torrey was born in New York City in 1796, and was the son of Captain William Torrey, of the Continental army, from whom he inherited the much-prized eagle of   **TORREY** Cincinnati. His mother was also of an old New

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[1] Powell. *op. cit.*, p. 184.

[2] *Idem.* p. 184.

[3] Biographical Memoirs of the [National Academy of Sciences, vol. 11, p. 267. Washington, 1886. John Torrey, by Asa Gray. In addition to the foregoing a sketch of Torrey accompanied by an engraved portrait on wood is contained in the *Popular Science Monthly*, vol. iii. p. 632. Also his portrait can be found in A History of the New York Academy of Science, by Herman Leroy Fairchild, New York, 1887.

York family. The boy was educated in his native city, and from Amos Eaton he learned "the structure of flowers and the rudiments of botany."[1] An education must have a broadening influence, and as he grew in years his interest in botany extended to chemistry and mineralogy, and finally to medicine, in which he was graduated from the College of Physicians and Surgeons in 1818. The practice of his chosen profession was not altogether congenial to him, and turning again to botany he began his Flora of the Northern and Middle United States. He published a portion of this work in 1824, and then accepted an appointment as assistant surgeon in the United States Army in order to become professor of chemistry, mineralogy, and geology in the United States Military Academy in West Point.

His abilities as a teacher received ample recognition, for in 1827 he was called to the chair of chemistry and botany in the College of Physicians and Surgeons, which he held until 1855. In 1830 he accepted the professorship in chemistry in Princeton, which he retained until 1854. These various collegiate appointments were then made *emeritus*, for on the establishment of the United States Assay Office in New York, in 1853, he was called to the charge of that place and held it until his death, twenty years later.

Gray says :

> It must not be forgotten that he was for more than thirty years an active and distinguished teacher, mainly of chemistry, and in more than one institution at the same time ; that he devoted much time and remarkable skill and judgment to the practical applications of chemistry, in which his counsels were constantly sought and too generously given.[2]

The foregoing quotation becomes especially significant when we remember that his botanical work, yet to be referred to, was accomplished in the intervals of his busy life. In 1836 he was appointed botanist to the state of New York, and in 1843 issued the two quarto volumes of which it has been so well said : " No other state of the Union has produced a flora to compare with this."[3] Prior to the organization of the special scientific bureaus in Washington, with their large staffs of competent specialists, it was the practice of the government

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[1] Gray. *op. cit.*, p. 268.

[2] *Idem*, p. 273.

[3] *Idem*, p. 271.

to refer the material collected by exploring expeditions to those most competent to report on it, and the botany in those years for the most part was assigned to Torrey. He reported on the specimens collected by Captain John C. Fremont in the expedition to the Rocky Mountains in 1845; on the plants gathered by Major William H. Emory on the reconnoissance from Fort Leavenworth, Missouri, to San Diego, California, in 1848; on the specimens secured by Captain Howard Stansbury on his expedition to the Great Salt Lake of Utah, in 1852; on those collected by Colonel John C. Fremont in California, in 1853; on those brought back from the Red River of Louisiana,. by Captain Randolph B. Marcy, in 1853; and those obtained by Captain Lorenzo Sitgreaves on his expedition to the Zuñi and Colorado Rivers, in 1854. Then followed his elaborate memoirs on the botany of the various expeditions connected with the Pacific Railroad Survey during the years 1855–1860; the Mexican Boundary Survey in 1859, and the Colorado River Expedition in 1861. It was this succession of magnificent monographs on the flora of North America that gained for him an imperishable reputation among the greatest of American botanists.

His associates have honored his name by giving it to certain species of shade trees, and so all round the world *Torreya taxifolia*, *Torreya californica*, *Torreya nucifera*, and *Torreya grandis* preserve his memory as green as their own perpetual verdure.[1]

Among our honorary fellows is the name of one who was not only a founder[2] of the Association of American Geologists in 1840, but also a founder of our own Association, and until the meeting of the American Association in Boston last year,

[1] Gray, *op. cit.*, p. 276.

[2] The following quotation concerning the formation of the Association of American Geologists is given in a sketch of Professor James Hall accompanied by an engraved portrait on wood that appeared in the *Popular Science Monthly*, vol. XXVI, p. 122, November, 1884 : " The comparison of observations and interchange of views led to the opening of correspondence, by a formal resolution of the New York Board, with other geologists, especially with those engaged in state surveys, of which several were then in progress. This correspondence led to an agreement for a meeting of geologists in Philadelphia in the spring of 1840, and this assemblage, of less than a score of persons, led to the organization of the Association of American Geologists, which, at a later period, on the occasion of its third meeting, added the term Naturalists; and, finally by expanding its title, it became the American Association for the Advancement of Science."

our senior past president. I refer, of course, to that nestor of American geologists, James Hall. It was but natural that he should be called to preside over the meeting in the city of his chosen residence. The second Albany meeting was held late in August, in 1856, and must ever remain a memorable one in the annals of American science on account of the inauguration of the Dudley Observatory at that time. It is no purpose of mine to consider the unfortunate controversy that followed that event, involving as it did the names and reputation of four great presidents of our Association—Henry, Bache, Peirce, and Gould, but no student of the history of American science can well ignore its existence.

Hall was born of English parents in Hingham, Massachusetts, in September, 1811, and after the usual schooling was about to prepare himself for the medical profession, when in **HALL** 1831 his interest turned toward natural science and he entered the Rensselaer School in Troy, where he was graduated in 1832. It was there that he came under the influence of Amos Eaton, and, like Torrey, profited by it. His connection with the Rensselaer Polytechnic Institute did not cease on graduation, for he was made an assistant, and later became professor of geology, which chair he retained until 1876, when he was made *emeritus*. Thus his loyalty to his *alma mater* continued for nearly seventy years, and was only severed by his death.[1]

His real life-work, however, was in connection with the Geological Survey of New York, which was organized and divided into four divisions in 1836. Hall was made an assistant geologist and assigned to the second division under Ebenezer Emmons. A year later he was appointed one of the state geologists, and assigned to the charge of the fourth district. He began his explorations in the western part of the state, and from 1838 till 1841 prepared the annual reports of progress in the work of his district. His final report, issued in 1843, as Geology of New York, Part IV, contains, according to T. Sterry Hunt, a description "in a very complete and exhaustive manner the order and succession of the strata, their mineralogical and lithological characters, and the organic remains which they contain."[2]

[1] Biographical Record of Rensselaer Polytechnic Institute.
[2] T. Sterry Hunt in the American Cyclopaedia, vol. VIII, article James Hall.

James D. Dana

John Torrey

James Hall

Jeffries Wyman

Retaining the title of State Geologist, he was in 1843 given charge of the paleontological work of the state survey, and the results of his many years of study have been given to the world in the thirteen volumes of the Natural History of New York, which bear the subtitle of Paleontology. These volumes have received the well-deserved encomium of being "the most comprehensive work of the kind which any state or country in the world possesses."[1]

The first appropriation—$15,000—that was made for this work was with the understanding that it should be completed for that sum, but again and again as the work progressed Hall appealed to the legislature for additional funds for its completion, until in 1894, it was estimated that the entire work had cost the state over $1,000,000. His comprehensive studies on the paleontology of New York naturally demanded researches beyond the limits of the state, and these he extended westward to the Rocky Mountains. It is now generally admitted that his investigations "have served as the basis of all our knowledge of the geology of the Mississippi Basin."[2]

In 1855 he was offered charge of the paleontology of the Geological Survey of Canada, with the promise of succeeding Sir William E. Logan as director on the retirement of the latter. When he was about to accept, promises of more liberal appropriations from the legislature of New York, and the influence of many leading American scientists, including Louis Agassiz and James D. Dana, led to his declining the offer, a decision which as the promises were never realized, he came to regard as "the great mistake of his life."[3]

The splendid work which he did in New York led to the request for his services elsewhere, and he was appointed to the charge of the Geological Survey in Iowa, in 1855, and to that of Wisconsin in 1857, preparing reports on both of these surveys. The paleontology of several government exploring expeditions was referred to him for discussion, notably that of Fremont, that of Stansbury, that of the Mexican Boundary Survey, and that of King's exploration of the fortieth parallel.

1 *New York Times*, August 9, 1898.
2 T. Sterry Hunt in the American Cyclopaedia. See note 2, p. 20.
3 Biographical Record of Rensselaer Polytechnic Institute.

In 1866 the New York State Museum was reorganized and Hall was made director, a place which he then held until 1893, retaining, however, until his death the office of State Geologist. It was at our Buffalo meeting, in 1896, that special commemoration exercises were held in honor of the sixtieth anniversary of Hall's connection with the Geological Survey of the State of New York, and at that time papers were read descriptive of his work. W J McGee described him as the "founder of stratigraphic geology and applied paleontology in America."[1] Referring to Hall's study of the crystalline stratified rocks he also said : "It is not too much to say that the method was established by the New York Survey, and that it finds its best illustration in the classic fourth district ; here it was that American stratigraphic geology was founded."[2]

It was also Hall, who, according to Hunt, "laid the grounds for a rational theory of mountains, which must be regarded as one of the most important contributions to geological science."[3]

Of the honors that he received, both in this country and abroad, no mention is necessary, but it may well be recalled that he represented our Association at the International Geological Congress held in St. Petersburg, in 1897, of which body he was a founder and its president in 1876.

He died in August, 1898, and " the monument of the man himself is builded in the rocks of New York, a monument more enduring than bronze or gold."[4]

Our Association is not local to the United States, but American, and at its tenth meeting it was decided to hold the gathering, in 1857, in Montreal, Canada. For president of that meeting Jacob Whitman Bailey was chosen, but early in the year he died and the vice-president filled his place.

Bailey was born in Auburn, Massachusetts, and after a common school education, he entered the United States Military **BAILEY** Academy where he was graduated in 1832. He joined the artillery branch of the service, and for several years was stationed at various army posts. An early fondness for natural science was assiduously cultivated

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[1] *Science*, New Series, vol. IV, p. 706.

[2] *Idem*, p. 702.

[3] American Cyclopaedia, vol. VIII. See note 2, p. 20.

[4] Benjamin K. Emerson in *Science*, vol. IV, p 717.

during these years, and he soon returned to West Point, where he was given charge of the chemistry, mineralogy, and geology, and later became professor of these branches, an appointment which he then held until his early death.

Bailey was one of the very first in this country to apply the microscope to the study of minute forms of life, and his work on infusorial fossils and the algae gained for him a high place among contemporary scientists. He was a pioneer on the examination of the deep-sea soundings made by the United States Coast Survey, and his report on that subject is given in one of the early volumes of the Smithsonian Contributions to Knowledge. This series of publications also contains his papers on terrestrial microscopical organisms. His name is associated with many improvements in the construction of the microscope, and the indicator devised by him is one of his most valuable contributions to science.

He died too soon, but not until his work had "won the approval of naturalists throughout the world."[1]

The vacancy created by the death of President Bailey was filled by Alexis Caswell, who was the first vice-president of the Association. It is a matter of record that "he sustained the credit of his country on a foreign soil by his dignified presence and his manly eloquence to the great satisfaction of his associates."[2]

Caswell was born in Taunton, Massachusetts, in 1799, and his ancestors were among the first settlers in that place. His paternal grandmother was a direct descendant of Peregrine White, who was born on the May-   **CASWELL** flower in 1620. He was graduated at Brown University in 1822, standing first in his class, and then passed to Columbian University in Washington, where for five years he taught both the classics and mathematics. It was in Washington that he made his special studies in theology under the direction of Dr. William Staughton, president of the university.

In 1827, having resigned his chair, he became pastor of the Baptist congregation in Halifax, Nova Scotia, but a year later

[1] Smithsonian Report for 1857, p. 74.

[2] Memorial of Alexis Caswell, D.D., LL.D., with an engraved portrait on steel, p. 29, being a reprint of the Memoir by Joseph Lovering, presented before the American Academy of Arts and Sciences.

he relinquished that charge to return to Providence on an invitation from the First Baptist Church there. The chair of mathematics and natural philosophy in Brown, becoming vacant, it was at once tendered him, and promptly accepted. For thirty-five years he continued in charge of the scientific instruction in the college of his choice, and then after a few years' rest he was chosen its president, which place he held until 1872, when, on the fiftieth anniversary of his graduation, he resigned.

It was the development of the various departments of science in Brown University that gave Caswell his high reputation among his contemporaries, but he had other claims that were also worthy of recognition. During the winter of 1858–59 he delivered four popular lectures on Astronomy before the Smithsonian Institution that were deemed of such importance as to warrant their insertion in the annual report of that year. His contributions to the young science of meteorology were of permanent value. Beginning with the year 1831, he instituted a series of observations in Providence which he continued until 1868, that were "precise as regards temperature and pressure ; and including also much information on winds, clouds, moisture, rain, storms, the aurora," etc.[1]

Henry was so " impressed with the service which they would render to the progress of meteorological research "[2] that they were published by the Smithsonian in its quarto series of Contributions to Knowledge. Few observers continued their uninterrupted series of observations for so long a period, and in those early days of meteorology such material was of the utmost value.

I have spoken of his career as a teacher, and I have referred to his contributions to science. In closing this brief sketch it must be added that he was prominent in many walks of life, taking ever an active interest in the welfare of his fellowmen. " In every charitable movement he was foremost with practical advice and generous aid."[3]

" Few men have filled more eminent positions in the walks

[1] Joseph Lovering in Memoir, p. 27.
[2] John L. Lincoln in Memoir, p. 57.
[3] *New York Tribune*, Jan. 9, 1877.

of learning and science, and few pass away more cherished in scholarly remembrance than Alexis Caswell."[1]

The twelfth meeting of our Association was held in Baltimore, and over that gathering Jeffries Wyman, of Boston, was chosen to preside, but when the time for the meeting came Wyman was unfortunately absent in South **WYMAN** America.   John E. Holbrook, of Charleston, South Carolina, the vice-president, was likewise unable to be present, and the duties of presiding again fell upon the competent shoulders of Caswell.

Wyman[2] was born in Chelmsford, Massachusetts, in 1814. He was the son of a physician, and after graduating from Harvard in 1833, followed in the footsteps of his ancestors, and with his brother, who still lives, studied medicine, both taking their degrees in 1837.   Boston became his home, and several minor appointments came to him, notably, a course of lectures on comparative anatomy at the Lowell Institute, with the proceeds of which he visited Europe, where for two years he studied anatomy under the best masters both in Paris and London.

In 1843 he returned to Boston and soon was appointed to the chair of anatomy and physiology in Hampden-Sidney College, in Richmond, Virginia. . The duties of this appointment called him from Boston during the winter and spring months only, and so offered a pleasant change from the rigors of the severer weather in Boston.   Five years later he was called to succeed the celebrated John C. Warren in the Hersey chair of anatomy in Harvard Medical College, a congenial post, which he filled with honor until his death.   Asa Gray said:

He was one of the best lecturers I ever heard, although, and partly because, he was most unpretending.   You never thought of the speaker, nor of the gifts and aquisitions which such clear exposition were calling forth,—only of what he was simply telling and showing you."[3]

No better description of his work is needed than the follow-

[1] *Taunton Gazette*, January 9, 1877. .

[2] Biographical Memoirs of the National Academy of Sciences, vol. II, p. 75. Washington, 1886.   Jeffries Wyman, by A. S. Packard.   See also *Popular Science Monthly*, vol. VI, p. 355, where an engraved portrait on wood is given ; also see Anniversary Memoirs of the Boston Society of Natural History, Boston, 1880, where a lithograph portrait is given.

[3] Anniversary Memoirs, p. 175.

ing quotation from his sympathetic friend and biographer, Asa Gray, who said :

He began his work in Holden Chapel, the upper floor being the lecture-room, the lower containing the dissecting room and the anatomical museum of the college, with which he combined his own collections and preparations, which from that time forward increased rapidly in number and value under his industrious and skilful hands. At length Boylston Hall was built for the anatomical and the chemical departments, and the museum, lecture- and working-rooms were established commodiously in their present quarters ; and Prof. Wyman's department assumed the rank and importance which it deserved. Both human and comparative anatomy were taught to special pupils, some of whom have proved themselves worthy of their honored master, while the annual courses of lectures and lessons on anatomy, physiology, and for a time the principles of zoology, imparted highly valued instruction to undergraduates and others.[1]

It would require more knowledge than I possess to properly present to you abstracts of the magnificent memoirs on comparative anatomy that came from the pen of this leader in science. That task, fortunately for the world, has been performed by one who studied with him, and to the memoir presented before the National Academy of Sciences[2] by Packard, I beg to refer you for that full and adequate treatment which Wyman's work deserves. Two quotations may, however, be given to indicate their value. His study on the gorilla, according to Gray, "assured his position among the higher comparative anatomists."[3] His paper on the bull frog was described as the "clearest introduction to the most complex of animal structures."[4]

So great was his knowledge of anatomy that a single sentence from Oliver Wendell Holmes sums up fully his remarkable ability to develop a structure from a single bone.

In a memorial trial [he says] his evidence relating to the bones which had been submitted to great heat is of singular excellence as testimony, and his restoration of fragments is a masterpiece of accuracy and skill.[5]

Wyman was a man of delicate constitution, and as he advanced in years it became his settled custom to spend the winters in Florida. It was in that land of flowers that he began

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[1] Anniversary Memoirs, p. 172.

[2] Biographical Memoirs, vol. 11, p. 77.

[3] *Idem.* p. 97.

[4] Jeffries Wyman. Memorial Meeting of the Boston Society of Natural History. October 7, 1874. p. 24.

[5] Biographical Memoirs, p 96.

his archæological work by the examination of the shell heaps, then little known, but now recognized as existing at many places along our Atlantic coast. These he studied with much interest and prepared reports on them which were published by the Peabody Museum in Cambridge. Of this institution he was one of the founders and its first curator. His successor and the second curator of that institution, I need hardly add, is President Putnam.

Packard described Wyman's career in the following strong sentence :

Wyman began his life-work as a comparative anatomist and physiologist, and in his riper years ranked as an anthropologist of a high order, his wide range of biological studies peculiarly fitting him for doing work of an unusual degree of excellence in the science of man, which may well be regarded as the synthesis of the biological sciences.[1]

Lowell wrote of him :

" Wisely to teach, because more wise to learn ;
To toil for Science, not to draw men's gaze,
But for her lore of self-denial stern ;
That such a man could spring from our decays
Fans the soul's nobler faith until it burns."[2]

For the meeting in the year 1859 the city of Springfield, Massachusetts, was chosen, and to preside over that gathering Stephen Alexander, of Princeton, was selected by his colleagues.

Alexander was born in Schenectady, New York, in 1806. As a boy he was slender and delicate, fond rather of books than of outdoor sports, and being an excellent student, he was given a college education. **ALEXANDER** He was graduated at Union in 1824 with high honor, although only eighteen years of age. For several years he taught, and then made astronomical observations in Albany, the results of which were communicated to the Albany Academy.

In a sketch[3] by his successor at Princeton, the inference is made clear that the marriage of his sister in 1830 to Joseph Henry had much to do with the shaping of his scientific career,

[1] Biographical Memoirs, p. 77.
[2] *Atlantic Monthly*, November, 1874.
[3] Biographical Memoirs of the National Academy of Sciences, vol. 11, p. 249. Washington, 1886. Stephen Alexander, by Charles A. Young.

for he followed Henry to Princeton in 1832, and then entered the Theological Seminary as a student.

It was undoubtedly Henry's influence that a year later persuaded him to accept an appointment to a tutorship in the college, which, in 1834, was made the adjunct professorship of mathematics. In 1840 he was made full professor of astronomy, which place he then held until 1876, when he was made *emeritus*.

His scientific work was entirely connected with astronomy, and beginning with 1834, he observed most of the solar eclipses visible in the United States. In 1860 he was made chief of the party that went to Labrador under the auspices of the United States Coast Survey to observe the eclipse of that year, and again in 1869 he was a leader of the observation party sent to Ottumwa, Iowa. He was associated with Henry in his thermopile observations on sun spots in 1845, as well as in other astrophysical researches, and to quote from Young :

He observed four transits of Mercury, and in December, 1882, he closed the record of more than fifty years by a careful and satisfactory observation of the transit of Venus.[1]

It would be too much to claim that Alexander was a great scientist, but fifty-three years of earnest devotion to his professional duties added to his valuable contributions to the science of his choice, is a career worthy of high honors. It was well said of him at the time of his death, in 1883, that "American astronomy to-day owes much to his life and labors."[2]

The unwritten law of alternating the succession in the presidential chair from a representative of the physical sciences to one devoted to natural science, received an emphatic demonstration in the selection of Isaac Lea as the successor of Alexander. The searcher for truth in the remote distance of far-away skies gave place to the patient student of fresh-water shells.

Born in Wilmington, Delaware, in 1792, Lea was early influenced towards a fondness for natural history by his **LEA** mother, who was devoted to botany. At the age of fifteen the boy was sent to Philadelphia to enter mer-

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[1] Biographical Memoirs, p. 254.
[2] *Idem*, p. 259.

ISAAC LEA.

cantile business, and there met Lardner Vanuxem, the future geologist. The young men spent their leisure in long walks, in which they collected minerals and studied the geological features of the vicinity.

While they had but little time to ramble through the country — both being clerks in a mercantile establishment — still their puny collections had great interest to them. One of their extended excursions was to examine the coal mines then opened above Wilkesbarre, where they found the slates contained mollusca, which Mr. Lea, some forty years afterwards, described in the *Journal of the Academy of Natural Sciences*. On their return they walked from there over the Pocono Mountain through the Wind Gap, where Mr. Lea found a trilobite, the first they had ever seen, and thence down the Delaware River, observing the strata all the way.[1]

Then they learned of the Academy of Natural Sciences, and the influence of that institution which has been exerted for so much good among the young men in Philadelphia, was extended to them. Membership was accorded to them in 1815, and two years later Lea presented his first paper before that body.

In 1821 he married Frances, the daughter of Mathew Carey, the writer and publisher, and became a member of the firm of M. Carey & Sons, at that time one of the most extensive publishing firms in the United States. Thirty years later he retired from business and devoted the remainder of his life to the pleasure of following his scientific tastes.

His interest in mineralogy gradually extended to geology, and especially to paleontology, through which he acquired a special fondness for fresh-water and land shells, to the study of which he devoted first his leisure from business and then all of his time. The unios were specially attractive to him, and his first conchological paper, published in 1827, was a description of six new species of that genus.

From this beginning grew his many papers on that particular mollusk until his separate articles collected under the title of Observations on the Genus Unio, 1827–1874, formed thirteen quarto volumes, containing two hundred and eighty plates. Besides the foregoing, he wrote many papers on new

1 Biographies of American Naturalists. Isaac Lea. Bulletin of the U. S. National Museum, with an etched portrait, Washington, 1885, p. 8. Also see *Popular Science Monthly*, vol. XXXI, p. 404, July, 1887.

species of the Strepomatidae, Colimaceæ, and other forms, indeed, it has been computed that nearly two thousand new species were described by him, of which nearly one-half were unios. His entire bibliography includes almost three hundred titles.

To the few specimens originally collected for study were soon added others that were sent to him from all over the world, and his cabinet, unique of its kind in the world as far as the unios were concerned, was bequeathed by him to the United States National Museum. As a memorial to him it fills the large hall of the Smithsonian Institution, and to students the fruits of his years of devotion to science are ever available, thus carrying out in a practical way the injunction of Smithson's bequest to found an institution for " the increase and diffusion of knowledge."

Lea's first love of minerals also followed him through life, and he formed a valuable collection of gem stones. These, like his larger cabinet, have found a permanent home in the National Museum. His special interest in connection with gems was concerning inclusions in crystals, and upon this subject he contributed a number of valuable papers.

The Academy in Philadelphia chose him as its president, in 1858, and two years later he became president of our Association. He lived until 1886, and continued his interest in science until the last. One of the features of the Philadelphia meeting in 1884 was the reception given by Lea to the visiting scientists, both from our own Association and from the British Association, at his summer home.

The long struggle of cruel warfare between the North and the South prevented any meeting of our Association for five years subsequent to the gathering in Newport, and so it was not until 1866 that the members of the American Association were reunited in a meeting held in Buffalo. It was a happy coincidence that for that occasion a president had been chosen in 1860, who at that time was one of the most famous of southern scientists, and who, in consequence of the fortunes of war, turned his steps northward to win even greater laurels as president of Columbia University. I refer, of course, to Frederick Augustus Porter Barnard, the selection of whom

did credit alike to the men of science, whether from the north or from the south.

Barnard was born in Sheffield, Massachusetts, in 1809, and his ancestors settled in New England early in its history.[1] As a boy he learned the printer's art, but not to the neglect of his studies, for he **BARNARD** was graduated at Yale in 1828, standing second in his class. At once he began his work as a teacher in the Hartford grammar school, and also took up in Hartford the study of law under Jonathan Edwards. Two years later he returned to New Haven and was made a tutor of mathematics. At that time a severe illness produced a temporary deafness, and as that affliction was hereditary in his family, it led him to retire from Yale and to turn his attention to the education of the deaf and dumb, accepting first a call in an institution in Hartford, and in 1832 to one in New York City.

In 1837 he was invited to the University of Alabama, where he filled, first the chair of mathematics and natural philosophy, and later that of chemistry and natural history, remaining in Tuscaloosa until 1854. It was said of him, at that time, that he was "the best at whatever he attempted to do; he could turn the best sonnet, write the best love story, take the best daguerreotype picture, charm the most women, catch the most trout, and calculate the most undoubted almanac."[2] As further evidence of his versatility it may be mentioned that he edited two newspapers of opposite political opinions. It was also while in Tuscaloosa that he delivered his famous Fourth of July oration, beginning "No just cause for a dissolution of the Union in any thing that has hitherto happened; but the Union is the only security for Southern rights." While it enraged his colleagues greatly, "this oration, read in every part of the state, as it was within a week, presented the northern cause in an entirely new light in Alabama, and checked the rising spirit of rebellion for many years."[3]

1 See Memoirs of Frederick A. P. Barnard, D.D., LL.D., tenth president of Columbia College, by John Fulton, New York, 1896 Also *Popular Science Monthly*, vol. xi, p. 100, and *Scientific American*, vol. lviii, p. 327, May 25, 1889, both of which contain portraits.

2 Appleton's Annual Cyclopaedia, vol. xiv, p. 73. This was to given me originally by Mrs. Barnard.

3 Clipping from *The New York Tribune*, probably of July 6, 1856.

In 1854 he accepted a call to the chair of mathematics, natural philosophy, and civil engineering in the University of Mississippi, of which institution he became president in 1856, and chancellor in 1858. When the civil war closed the doors of that university he declined office under the Confederate government and came north. For a time he was connected with the United States Naval Observatory, and also with the United States Coast Survey, but the vacant chair of physics in Columbia College attracted him, and the trustees of that institution were wise in taking advantage of their opportunity to offer him the higher honor of the presidency of Columbia College, a place from which President King had just resigned. This office he accepted and then held until his death. Elsewhere[1] I have said :

His clear judgment, remarkable executive ability, and fondness for work resulted in the great development that has occurred in that institution since his connection with it. The School of Mines, perhaps the foremost scientific school in this country, was the first of the innovations to which he gave his earnest attention. The School of Political Science, the School of Library Economy, the department of women, known as the Barnard College, the gathering of the many departments into the magnificent buildings that now constitute almost a university at 49th street and Madison avenue, are largely due to him.

Newberry, who for so long was closely associated with him, in an admirable address, in which he presented a summary of Barnard's career as an educator, said of the growth of Columbia during his presidency :

He made there a noble and an honorable record. Every one of the steps of progress was either conceived or earnestly advocated by him and owed its achievement to his support. He was not only a participant, but a leader in every forward movement.[2]

To follow Barnard in his many spheres of activity would carry this address to undue length, but it should be remembered that he was the senior editor-in-chief of Johnson's Cyclopaedia, and his reports in connection with the World's Fairs held in Paris in 1867 and 1878, and in Philadelphia in 1876, were valuable contributions to science. It was said of him :

Among the promoters of science and liberal culture in our time, few

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[1] *Scientific American*, May 25, 1889.

[2] John S. Newberry. Necrology Report of the University Convention of the State of New York. (Reprint.)

men have labored more efficiently and successfully than the present versatile and accomplished president of Columbia College.[1]

In conclusion let me quote the lines that his friend Whittier wrote of him :

> Rich, from life-long search
> Of truth, within thy academic porch
> Thou sittest now, lord of a realm of fact,
> Thy servitors the sciences exact ;
> Still listening with thy hand on Nature's keys,
> To hear the Samian's spheral harmonies
> And rhythm of law.

As I approach that period in the history of our Association during which it has been my privilege to know personally the men who were our leaders, the pleasure of preparing this address increases. Barnard was president of Columbia during my undergraduate course there, and perhaps the last time that I saw him was on the occasion of the meeting of the American Association in New York, in 1887. Another meeting was yet to come and go, and then Barnard too was called away to join the silent majority.

In that admirable address with which he welcomed the Association to Columbia he reviewed the labors of his many distinguished predecessors in the Association, saying in conclusion :

All these have gone to their rest, many of them full of years, all of them full of honors. Others have risen to fill their places, no less earnest, no less capable, and destined to be no less illustrious.[2]

Among all of these there is none of whom I am prouder on this occasion and in this place to express my love and honor for than John Strong Newberry, **NEWBERRY** of whom it was so well said :

He is a geologist—keen of eye, stout of limb, with a due sense of the value of detail, but with a breadth of vision that keeps detail in due subordination.[3]

Newberry[4] was born in Windsor, Connecticut, towards the close of the year 1822. He was of early New England ancestry, and was specially proud of the fact that his grandfather

[1] *Popular Science Monthly*, vol. XI, p. 100.

[2] Proceedings, American Association for the Advancement of Science, vol. xxxvi, p. 342.

[3] Address made at the presentation to Newberry of the Murchison medal in 1888 by the Geological Society of London. He was the first American geologist to receive that honor.

[4] See sketch in *Popular Science Monthly*, vol. IX, p. 490, August, 1876, with an engraved portrait on wood, and also *Scientific American*, December 31, 1891, with half-tone portrait.

was an officer in the American army during the war of the Revolution. The boy was barely two years old when his parents moved to Ohio, and Cuyahoga Falls became his home. He was educated at Western Reserve College, and received his doctor's degree from the Cleveland Medical College in 1848, after which he spent two years in special study abroad.

Then settling in Cleveland he began the practice of medicine, but his love for natural science was greater than his fondness for his profession, and in 1855 he accepted an appointment in the United States Army as assistant surgeon. From that time until 1861 he served both in his professional capacity and as a geologist to exploring parties. At first under Williamson who was sent to examine the country between San Francisco and the Columbia River; then under Ives in his exploration of the Colorado River; and finally under Macomb with the expedition sent to study the San Juan and Upper Colorado rivers. On the material gathered during each of these expeditions he prepared valuable scientific reports, which were published by the government. In these volumes will be found pioneer work of great value, much of which has been lost sight of on account of the greater development of the same territory by subsequent expeditions. In an appreciative sketch of him by Kemp, his successor at Columbia, that appeared at the time of his death, I find this statement:

His determinations of strata in the west, although based on the hasty itineraries of exploring parties, have been very generally corroborated by later and more deliberate work.[1]

His wonderful "ability to grasp as by intuition the bearings of many widely separated facts,"[2] would have gained even greater renown for his early work in the west, had not the civil war intervened.

From 1861 to 1866 Newberry was secretary of the Western Department of the United States Sanitary Commission with supervision of all the operations of that body in the valley of the Mississippi. He organized the comprehensive work of the commission in the large section that was entrusted to his care, and during its life expended more than $800,000 in money, be-

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[1] In Memoriam. Professor John Strong Newberry, *School of Mines Quarterly*, vol. XIV, p. 90, January, 1893, with two engraved portraits on steel.

[2] *Idem*, p. 99.

sides distributing hospital stores that were valued at more than $5,000,000. The whole story of that wonderful achievement, its development, and its completion was told by himself in his report of the commission in the valley of the Mississippi that was published in 1871.

With the return of peace came a new interest in the development of our institutions of learning, and conspicuous among the newer experiments was the then recently organized School of Mines of Columbia College. It was the first institution of its kind in the United States, and its success was yet to be determined. Newberry was called to the charge of the department of geology in the new school, and with a faith in its ultimate success that never faltered, he accepted the trust. With the same genius for organization that was shown by his development of the work of the Sanitary Commission, he began the planning of courses of study. Alone he gave instruction in botany, zoology, geology, lithology, paleontology, and economic geology, and a quarter of a century later left to the world as his best and greatest memorial a magnificently equipped department of the special branches taught by him not excelled by any similar educational institution in this country. Nor was this all. He created a museum of over 100,000 specimens, principally collected by himself, which served to illustrate his lectures on geology and economic geology. It contains "the best representatives of the mineral resources of the United States to be found anywhere, as well as many unique and remarkable fossils."[1]

Kemp calls it "a monument to his memory," and adds:

Its wealth of fossil fish and fossil plants makes it unique and famous among geological museums.[2]

In 1869 he became state geologist of Ohio, and for many years he regularly spent his summers in the field, while the accumulated material was digested during the winter months in the laboratory in New York, yielding the nine large volumes of reports published by Ohio. The unwillingness of the State Legislature to permit the completion of the work as originally intended, was the great grief of his closing years, and marked the beginning of his end.

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[1] This is his own description taken from a personal letter written to me in 1888.
[2] *School of Mines Quarterly*, vol. xiv, p. 99.

It is a pleasure to remember that during the last years of his life he received the fossil plants and fishes from the United States Geological Survey to report on, and so returned to the study of those forms which, as a boy, he loved to collect in the coal deposits of eastern Ohio.

He was rich in those accumulated experiences that we call wisdom. He was a friend, faithful and true, as those who knew him can testify. He is gone, but his influence cannot die. It will live forever to "reach thro' nature, moulding men."

American astronomers hold a high place in the history of the development of their chosen science, and among those in our country who have made the study of the heavens their chief life-work, first place must unquestionably be given to Benjamin Apthorp Gould for his splendid achievements. In his time he ranked as the greatest of our astronomers, and our Association honored itself in choosing him to preside over the meeting held in Chicago in 1868.

Gould[1] was born in Boston in 1824, and was graduated with honors at Harvard twenty years later. He then **GOULD** went abroad and for four years studied under the most distinguished astronomers of Europe, but chiefly under the great Gauss, in Göttingen, where he received his doctor's degree.

In 1848 he returned to Boston, and there—a little more than half a century ago—began the publication of the *Astronomical Journal*, the first and still the only distinct periodical of that science devoted to original investigation in this country.

Then came his valuable connection with the Coast Survey, during which he had charge of the longitude determinations, and subsequent to the laying of the Atlantic cable in 1866, he connected the two continents by precise observations. These first determinations of transatlantic longitude by telegraph were the means of establishing a connected series of longitude measurements from the Ural Mountains to New Orleans. In the successful accomplishment of this work he anticipated his English colleagues, and so added greater renown to the advancement of American science.

From 1856 to 1859 he was director of the Dudley Observa-

[1] See sketch with engraved portrait on wood in *Popular Science Monthly*, vol. **xx**. p. 683. March, 1882.

tory in Albany, and superintended its construction. It was in this building that the normal clock, protected from atmospheric variations and furnished with barometric compensation, was first used to give time telegraphically to dials throughout the observatory ; indeed, as improvements of his own suggestion were established, the service was extended until it was that clock that gave the time signals to New York. The three years of his valuable services to science at Dudley were marred by a famous controversy, the discussion of which cannot be taken up here. It had to do with the important question as to whether the wishes of a board of trustees should be carried out by a scientific director. Gould absolutely declined to accept the dictates of those who determined to compel him to adopt a policy which was opposed to that which he regarded as best for the scientific development of the observatory. Firm in his belief as to what was right, he declined to resign, and finally, by process of law, was removed from his directorship. Gould fought his fight bravely and honestly, and though in the end he was unsuccessful, still to his credit it must be said, he never yielded his ground.

Soon after the beginning of the civil war he was invited by the United States Sanitary Commission to take charge of the compilation of their statistics, and he organized an elaborate system of anthropological measurements, which, under his direction, were computed and tabulated, yielding a thick volume entitled Investigations in the Military and Anthropological Statistics of American Soldiers.

The great event of his life was the magnificent work accomplished by him while director of the National Observatory of the Argentine Republic in Cordoba. In 1868 he was called to the organization of the observatory there, and after obtaining from Europe a complete outfit of instruments, superintended the erection of the observatory.

He began work in 1870. Of the work accomplished he said :

The original purpose was to make a thorough survey of the southern heavens by means of observations in zones between the parallel of 30° and the polar circle ; but the plan grew under the influence of circumstances, until the scrutiny comprised the whole region from the tropic to within 10° of the pole—somewhat more than 57° in width, instead of 37°. Although it was no part of the original design to perform all the

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numerical computations, and still less to bring the results into the form of a finished catalogue, it has been my exceptional privilege, unique in astronomical history so far as I am aware, to enjoy the means and opportunity for personally supervising all that vast labor, and to see the results published in their definite, permanent form.[1]

It was also under his direction that the Argentine Meteorological Service was established in 1872, and its work he described as follows:

At the end of the year 1884 there were already twenty-three points at which the observations had been continuously made, three times a day, for at least four years, and sixteen others at which they had already been continued for more than two years. These have provided the necessary data for constructing the isothermal lines, with tolerable precision, for all of South America from the torrid zone to Cape Horn.[2]

His work done, and well done, he came home to pass the evening of his life with the friends and associates of his early years. His return to the United States was celebrated by a dinner, at which those who knew him best, greeted him with glad words of welcome. Holmes wrote for that occasion:

> Once more Orion and the sister Seven
>   Look on thee from the skies that hailed thy birth—
> How shall we welcome thee, whose home was Heaven,
>   From thy celestial wanderings back to earth?
>
> Fresh from the spangled vault's o'erarching splendor,
>   Thy lonely pillar, thy revolving dome,
> In heartfelt accents, proud, rejoicing, tender,
>   We bid thee welcome to thine earthly home.[3]

While George S. Hale, a classmate, greeted him with

> The wanderer we welcome home,
>   From far-off lands to us unknown,
> Which see, with pride, his name displayed
>   On their bright skies, thus made his own.
>
> But not alone "The Southern Crown"
>   Shall cast its halo round his head;
> The stars he worshipped in his youth
>   Their shining welcome o'er him shed.[4]

Advancing years came pleasantly to him. In Cambridge he reestablished the *Astronomical Journal*, the special pride of his

1 Addresses at the Complimentary Dinner to Dr. Benjamin Apthorp Gould, p. 15.
2 *Idem*, p. 17.
3 *Idem*, p. 22.
4 *Idem*, p. 32.

early life, and honors, such as are accorded only to the very great, came to gladden him with their special significance of recognition and appreciation. A dozen peaceful years were spent in the quiet of his own home before the end came, and then he passed beyond the stars to his new home in the far-away skies.

The meeting in Chicago brought into conspicuous notice one of the pioneers in American geology, whose fine attainments had been honored locally by his election to the presidency of the Chicago Academy of Sciences. Our Association was quick to recognize the growing advancement of science in the west by electing John Wells Foster to preside over the Salem meeting in 1869.

Foster was born in Petersham, Massachusetts, in 1815, and was a lineal descendant of Myles Standish, of Mayflower celebrity. He was educated at Wesleyan University, and then studied law. In the early thirties **FOSTER** Ohio was still the El Dorado of New England, and Foster settled in Zanesville, where he completed his law studies and was admitted to the bar.

For a time he practiced his profession, but the rich discoveries of mineral wealth in the western territories led to his deciding upon civil and mining engineering as the most desirable life-work for him to pursue. He soon became a recognized authority, and was employed regularly to examine and report on mining regions. He served also on the Geological Survey of Ohio, making a valuable report on the coal fields of that state.

In 1847 the national government instituted a geological survey of the Lake Superior region, which at that time was attracting much attention, owing to the discovery of the copper deposits there. Charles T. Jackson was appointed in charge of the expedition, and he chose as his assistants Foster and Josiah D. Whitney. On the completion of the work, two years later, the preparation of the report was assigned to the younger men. The two slender volumes were published by Congress, and still remain the accepted authority on the subject of which they treat. It was at the Cincinnati meeting of our Association in 1851, that the elder Agassiz "declared it

to be one of the grandest generalizations ever made in American geology."

He returned to Massachusetts and was active in politics, serving for some years as one of the Governor's executive council, but in 1858 he again went west, and Chicago became his permanent home. For some years he had charge of the land department of the Illinois Central Railroad, and then held a similar connection with the Chicago & Alton Railroad, but he relinquished these appointments to return to the pursuit of science, and accepted a chair of natural history in Chicago.

He was the author of The Mississippi Valley, Its Physical Geography, which gave valuable sketches of the topography, botany, climate, and geology of that part of the United States. His last work, published shortly before his death, was on Prehistoric Races of the United States, and gave the results of his investigations of the mounds found in various places in the Western States. He was the editor of the *Lakeside Monthly*, and a frequent contributor to literary and scientific periodicals. It was said of him that "his varied experience, his wide and accurate knowledge of facts, his intellectual comprehensiveness, and discriminativeness made him the peer of the foremost scholars of his time, while his personal and social qualities made him respected and loved by all who came within the radius of his winning personality." He died in 1873.

The gathering in the west was succeeded by one in the east, and Troy, New York, was selected as the meeting place of our Association in 1870. William Chauvenet was **CHAUVENET** chosen to preside, but as the time came for the gathering of the scientists his health was so precarious, and his end so near, that he was unable to be present, and the vice-president, Thomas Sterry Hunt, occupied the chair. Both names are included in the list of our presidents, and a brief sketch of each is therefore given.

Chauvenet[1] was born in Milford, Pennsylvania, in 1824, and was graduated at Yale in 1840. The mathematical ability that he had shown while in college led to his prompt appoint-

[1] Biographical Memoirs of the National Academy of Sciences, Washington, 1856. vol. 1. p 227, William Chauvenet, by J. H. C. Coffin.

ment as assistant to Alexander D. Bache, who gave him charge of the reduction of the meteorological observations then being carried on at Girard College. A year later, however, in 1841, he received an appointment as professor of mathematics in the United States Navy, and continued in that capacity until 1859. At first he served on board of the steamer *Mississippi*, and later at the Naval Asylum, in Philadelphia, but he became greatly interested in the proposed establishment of the United States Naval Academy, in Annapolis, and when that institution became a reality he was transferred there, receiving the chair of astronomy, navigation, and surveying, and was "always the most prominent of the academic staff."[1]

It was while connected with the navy that Chauvenet prepared that valuable series of mathematical text-books that made his name known throughout the world. They included a book of Logarithms, a Treatise on Trigonometry, and Manual of Astronomy, and while they have been the delight of many students, they have also been the terror of those to whom the study of mathematics was uncongenial.

In 1855 the chair of mathematics, and in 1859 the chair of astronomy and natural philosophy, at his *alma mater*, were offered to him, but the rigors of the northern winters he feared would be too severe for his delicate constitution, and he declined to accept either of them. But in the last-named year he was called to the professorship of mathematics in the then recently founded Washington University in St. Louis, and in 1862 he was made chancellor of that university, but two years later failing health compelled him to abandon all active work, and he sought recuperation in travel. In 1865, with apparently restored health, he was able to practically resume his duties, but four years later he was obliged to relinquish them entirely. It was at that time that he was elected to the presidency of our Association, but he was unable to attend the meeting, and in December, 1870, he died in St. Paul, Minnesota. Mention should be made of the fact that he served the Association as general secretary at the Springfield meeting in 1859.

There have been men of extraordinary ability, there have

[1] Biographical Memoirs, p. 235.

been men of great talents, and there have been famous students who have laboriously worked out important discoveries, among those who have held the high office of president of our Association, but among them all, two only, Hunt and Cope, it seems to me, possessed those brilliant mental qualities which are the natural endowments of genius.

Hunt[1] was born in Norwich, Connecticut, in 1826, and was descended from William Hunt, one of the founders of Concord, Massachusetts, in 1635. His maternal grandfather was Consider Sterry, of Norwich, a well-known mathematician and civil engineer in his time. His early education was slight, but as a young man he became laboratory assistant in the chemical department of Yale under the elder Silliman. Seldom has an opportunity been used to greater advantage, and so quickly did he acquire a knowledge of the sciences presented, that after two years in New Haven he was, in 1847, appointed chemist and mineralogist to the Geological Survey of Canada, a place which he then held for exactly a quarter of a century. During that period, with his unusual powers, he presented to the scientific world those remarkable contributions to the twin studies of chemistry and geology that have gained for him a foremost place among the pioneers of the newer science of geological chemistry. His early papers treated of chemistry. He developed a system of organic chemistry in which all chemical compounds were shown to be formed on simple types represented by one or more molecules of water or hydrogen.[2] He anticipated Dumas with his researches on the equivalent volumes of liquids, and in 1887 published in book form, under the title A New Basis for Chemistry, a full digest of his papers, forming a complete system of his theory of chemistry.

HUNT

Of his contributions that pertained more especially to geology, I must quote from a sympathetic writer, who said :

To Dr. Hunt we owe the first systematic attempt ever made to sub-

1 See *Popular Science Monthly*, vol. viii, p. 486, February, 1876, with an engraved portrait on wood. See also sketch with half-tone portrait in *Engineering and Mining Journal*, November 7, 1891, and sketch by R. W. Raymond in that Journal for February 20, 1892. The *Scientific American* of March 19, 1892, likewise contains a sketch of Hunt with a half-tone portrait.

2 See a Century's Progress in Chemical Theory, *American Chemist*, vol. v, p. 56, August, 1874.

divide and classify geologically the stratiform crystalline rocks ; a work
to which he brought not only his studies throughout Canada and the
United States, but those made during repeated visits to the British Islands
and to continental Europe.   To him we are indebted for the distinctions
and the designations of Laurentian, Norian, Huronian, Montalban, Ta-
conian, and Keweenian, all of which have now passed into the literature
of geology.   In connection with these studies he attempted the discussion
of the great questions of the origin and the succession of these rocks.
Reviewing and controverting various hypotheses, including the igneous
or plutonic, the metamorphic and the metasomatic, all of which are re-
jected as irreconcilable with observed facts, and as violating chemical
theory, Dr. Hunt vindicated the essential soundness of the still imperfect
Wernerian aqueous view, and advanced what he named the crenitic hy-
pothesis.   According to which, the source of the various groups of crys-
talline rocks above named was the superficial portion of a globe, once in
a state of igneous fusion, but previously solidified from the center.   This
portion, rendered porous by cooling, was permeated by circulating waters,
which dissolved and brought to the surface during successive ages, after
the manner of modern mineral springs, the elements of the various sys-
tems of crystalline rocks.   These thus mark progressive and necessary
changes in the mineralogical evolution of the earth during the pre-Cam-
brian or Archaean ages.[1]

Hunt himself described his views as follows :

The new hypothesis is the result of nearly thirty years of studies, hav-
ing for their object to reconstruct the theory of the earth on the basis of
a solid nucleus, to reconcile the existence of a solid interior with the
flexibility of the crust, to find an adequate explanation of the universally
contorted attitude of the older crystalline strata, and at the same time to
discover the laws which have governed the formation and the changing
chemical composition of the stratiform crystalline rocks through success-
ive geological ages.[2]

In 1872 he returned to the United States and accepted the
chair of geology in the Massachusetts Institute of Technology
made vacant by the retirement of William B. Rogers, and re-
mained in that capacity until 1878, after which New York
City became his principal home, and he devoted his leisure,
until his death, in perfecting his books, which present in ma-
tured form the opinions originally published as addresses or
special papers.   They include Chemical and Geological
Essays; Mineral Physiology and Physiography; and Sys-
tematic Mineralogy According to a Natural System, and
according to R. W. Raymond, ''constitute a monument

1 *Engineering and Mining Journal*, November 7, 1891.
2 *Idem*, November 7, 1891.

to his genius, industry, and learning which certainly cannot be overlooked by the historian of science.''[1]

Three times during the life of our Association has the science of botany been conspicuously honored by the selection of its most distinguished representative to preside over one of our meetings. The first of these occasions was in 1855 when the able Torrey filled the presidential chair with much grace and dignity, and the second was at the Indianapolis meeting in 1871, when Asa Gray was the presiding officer.

Gray[2] was born in the Sauquoit Valley, in New York, in 1810, and was the son of a farmer. At an early age he showed a greater fondness for reading than for duties around the farm, and his father wisely decided to make a scholar of him. He was sent to school in Clinton, New York, and later to an academy in Fairfield, New York. At the last-named place in **GRAY** compliance with the desires of his father he entered the medical school, and in 1831 received his doctor's degree from that institution. Meanwhile, however, he acquired an interest in natural science, largely through the influence of Dr. James Hadley, the professor of materia medica and chemistry. Farlow says ''he was not at first so much interested in plants as in minerals,''[3] and this is of special interest, for it was about that time that he first met Dana, with whom he ever afterward maintained a close friendship.

It is also Farlow who is my authority for the statement ''that his passion for plants was aroused by reading the article on Botany in the Edinburgh Cyclopaedia,''[4] and with a fondness for collecting, we learn that even before graduating ''he had brought together a considerable herbarium.''[5]

It does not appear that he ever practiced medicine, for during the same year that he graduated he became instructor in chemistry, mineralogy, and botany, in the high school in

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[1] *Engineering and Mining Journal.* February 20, 1892.

[2] See Memorial of Asa Gray reprinted from the Proceedings of the American Academy of Arts and Sciences, and Biographical Memoirs of the National Academy of Sciences, vol. III, p. 161, Asa Gray, by W. G. Farlow. See also Letters of Asa Gray, by Mrs. Jane Loring Gray, 2 vols. Boston, 1893; and Scientific Papers of Asa Gray, selected by Charles S. Sargent. 2 vols. Boston, 1888.

[3] Memorial of Asa Gray. p. 20.

[4] *Idem.* p. 20.

[5] *Idem.* p. 20.

J. W. Foster.

Utica, and he also lectured on these subjects at the medical school.

In 1833 he went to New York, where he joined Torrey, whose assistant he became, and two years later, through Torrey's influence, he was appointed curator and librarian of the Lyceum of Natural History, now the New York Academy of Sciences. About that time the preliminary arrangements for the Wilkes Exploring Expedition were being made, and the place of botanist was accepted by Gray. It was the fact that his friend Gray had accepted an appointment on the expedition that led Dana to consider favorably an invitation to serve as its mineralogist. However, the departure of the expedition was delayed for some time, and in the meanwhile Gray resigned to accept a closer relationship with Torrey, who sought his association in the preparation of his Flora of North America.

The organization of a great university is in many ways a formidable undertaking, and the selection of its faculty is perhaps the most difficult of all the problems that come up for consideration. Some sixty years ago the University of Michigan elected Asa Gray as its first professor of botany. He accepted the honor, but asked that he be permitted first to spend a year abroad in study. The splendid opportunities for settling disputed points in American botany, as well as the association with many students of science who have since become eminent, was fruitful of rich results, and so it was that on his return the continuation of the Flora demanded his first attention. The young university in the west lost his services, but botany, as a science, was the gainer. Later, perhaps, he might have settled in Ann Arbor, but in 1842 an opportunity, such as comes to but few men, came to him when he was invited to accept the Fisher professorship of Natural History in Harvard. At that time " there was no herbarium, no library, only one insignificant greenhouse, and a garden, all in confusion with few plants of value."[1] To describe the development of the botanical department of Harvard, as guided by him, would take more space than I can rightly give, and in this case it is not necessary to attempt it, for in the Memorial of

[1] Memorial of Asa Gray, p. 26.

Asa Gray, from which much has already been taken, the
story is told by his three friends and associates, Goodale, Wat-
son, and Farlow, each of whom succeeded to a share of his
work. I may, however, say that at · the time of his death, in
1888, the herbarium, the largest and most valuable in America,
contained over 400,000 specimens, the library had more than
8,000 titles, the "insignificant greenhouse" had been increased
many fold, and the garden had become the most important of
its kind in this country. President Eliot said :

He had placed on a firm foundation the botanical department of the
university which he served for forty-six years, and that the collections he
had created there would have for generations a great historical import-
ance.[1]

Like Louis Agassiz, Wolcott Gibbs, Jeffries Wyman, and
other of his great contemporaries at Harvard, his influence as
a teacher was remarkable, and it was well said of him that
" he trained up a whole race of botanists, now scattered through
all parts of the United States."[2] Like Dana, his influence
was extended by his text-books throughout the English-speak-
ing world. His Elements of Botany, first published in
1836, became later the Structural and Systematic Botany.
The well-known Manual of the Botany of the Northern
United States is still a classic. How Plants Grow and
How Plants Behave found their way where botany as
botany could not have gained an entrance, and they set in mo-
tion a current which moved in the direction of a higher science
with a force which can hardly be estimated."[3]

Reference has already been made to the joint publication of
the Flora of North America, by Torrey and Gray, and
with the completion of the part devoted to the compositæ, that
work ceased to appear and was never resumed, but for many
years after his settling in Cambridge the valuable acquisitions
of the government expeditions were in part referred to Gray,
and in this way he continued his splendid work on American
botany. Subsequent to 1873, when more active duties of his
chair at Harvard was surrendered to his able assistants, he
began the preparation of a Synoptical Flora of North Amer-
ica, only part of which he lived to finish.

1 Memorial of Asa Gray, p. 9.
2 *Idem*, p. 28.
8 *Idem*, p. 32.

My sketch of Gray has outgrown the limits originally assigned to it, but I cannot close it without making mention of the fact that he "was probably the best expounder of Darwinian principles" among American naturalists. This is of special significance when it is remembered that Gray was a staunch adherent of the Presbyterian Church, the orthodox faith of his ancestors. To those who care to study his views more fully I can heartily commend his collected essays and reviews contained in his Darwiniana published in 1876, and his admirable lectures on Natural Science and Religion, delivered before the theological school of Yale University in 1880.

In conclusion let me quote the words of Dr. J. E. Sandys, of, Cambridge, who, in conferring the degree of Doctor of Science from that famous old university, said :

This man who has so long adorned his fair science by his labors and his life, even unto a hoary age, "bearing," as the poet says, "the white blossoms of a blameless life," him, I say, we gladly crown, at least with these flowerets of praise, with this corolla of honor. For many, many years may Asa Gray, the venerable priest of Flora, render more illustrious this academic crown !![1]

Sir Joseph D. Hooker said of Gray :

When the history of the progress of botany during the nineteenth century shall be written, two names will hold high positions, thoSe of Professor Augustin Pyramus de Candolle and of Professor Asa Gray . . . Each devoted half a century of unremitting labor to the investigation and description of the plants of continental areas, and they founded herbaria and libraries, each in his own country, which have become permanent and quasi-national institutions. . . . There is much in their lives and works that recalls the career of Linnaeus, of whom they were worthy disciples, in the comprehensiveness of their labor, the excellence of their methods, their judicious conception of the limits of genera and species, the terseness and accuracy of their descriptions, and the clearness of their scientific language.[2]

The brilliant work in chemistry done by J. Lawrence Smith, combined with the fact that prior to his election no representative of chemistry had ever been chosen as president of our Association, had doubtless much to do with his selection to preside over the gathering held in Dubuque, Iowa, in 1872. The

[1] Asa Gray, by Walter Deane, with an electrotype portrait, *Bulletin of the Torrey Botanical Club*, vol. XV, p. 70.

[2] Memorial of Asa Gray, p. 34.

wisdom of the choice was confirmed early in that year by his election to the National Academy of Sciences.

Smith[1] was born in Charleston, South Carolina, in 1818, and studied civil engineering at the University of Virginia, but preferring medicine, he was graduated in 1840, at the Medical College in Charleston, submitting as his thesis a valuable paper on The Compound Nature of Nitrogen. As was largely the custom in those days, he spent several years in Europe, passing his winters in Paris, where he studied chemistry with Dumas, toxicology with Orfila, and physics

SMITH with Becquerel, and his summers in Giessen studying with the immortal Liebig. While he was in Paris the celebrated poison case of Madame La Farge occurred, in which the question of the normal existence of arsenic in the human system was involved, and although he was a student under Orfila, he did not hesitate to differ with his master and review the entire question in a paper, in the conclusion of which in after years, Orfila himself agreed. It was in that way that his interest in medicine became subordinate to that of chemistry.

In 1844 he returned to Charleston, where he entered on the practice of his profession, and during the winter delivered a course of lectures on toxicology in the medical college.

The development of mineral wealth in the different states was beginning to be considered an important matter, and in South Carolina Smith's recognized ability and education led to his appointment as state assayer to test the bullion coming into commerce from the states of Georgia and the two Carolinas. This place he accepted, and so relinquished his practice.

It naturally followed that he should devote some attention to agricultural chemistry, and the great marl beds on which the city of Charleston stands attracted his notice. It was he who "first pointed out the large amount of phosphate of lime in these marls, and was one of the first to ascertain the scientific character of this immense agricultural wealth.[2] Dr. Smith also made a valuable and thorough investigation into

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[1] Biographical Memoirs of the National Academy of Sciences. Vol. II, p. 217. John Lawrence Smith, by Benjamin Silliman, with a Bibliography. See also Original Researches in Mineralogy and Chemistry, by J. Lawrence Smith, Louisville, 1884. This memorial volume contains several biographical sketches and a portrait of Dr. Smith.

[2] Dr. J. B. Marvin in Original Researches, etc., p. 10.

the meteorological conditions, character of soils, and culture affecting the growth of cotton.

This work attracted considerable attention, and in consequence he was regarded by James Buchanan, then Secretary of State, "as a suitable person to meet the call from the Sultan of Turkey for scientific aid in introducing into that kingdom American methods in the culture of cotton."[1] On reaching Turkey he found that a commission was already engaged on the problem of cotton culture, and as he was about to return, the Turkish government invited him to report on the mineral resources of its territory. This work proved most valuable, and his discoveries of emery deposits in Asia Minor destroyed the monopoly then held by the Island of Naxos. Says Dr. Marvin :

His studies in emery and its associated minerals led directly to its discovery in America. In Massachusetts and North Carolina, a large industrial product of emery is now being carried on. To Dr. Smith justly belongs the credit of having done almost everything for these commercial enterprises by his successful researches in emery and corundum.[2]

In 1850 he returned to the United States, and for two years lectured on science in New Orleans, and was elected professor of chemistry in the University of Louisiana, an institution which he said "at present exists but in name." Two years later he was called to succeed Robert E. Rogers in the chair of chemistry in the University of Virginia, and then began with George J. Brush that splendid series of analyses of American minerals. Silliman said of them : "They settled many doubtful points and relegated into obscurity many worthless theories, while clearly establishing others."[3]

His stay at the University of Virginia was a short one, for at the end of the year he resigned and settled in Washington, where he became connected with the Smithsonian Institution as chemist, also devoting some attention to agricultural chemistry for the Department of Agriculture.

Louisville was the home of his wife's family, and the chair of medical chemistry and toxicology in the University of Louisville, made vacant by the resignation of the younger Silliman,

[1] Silliman in Original Researches, etc., p. 27.
[2] Original Researches. p. 11.
[3] *Idem*, p. 33.

was tendered to him in 1854. That place was promptly
accepted, and thereafter Louisville became his home. For
twelve years he continued .his professorial duties, and also
manifested his fondness for practical chemistry by his accept-
ance of the charge of the Louisville Gas Works, and by his
establishing with the venerable Dr. Edward R. Squibb a labor-
atory for the production of chemical reagents and the rarer
pharmaceutical preparations.

It was during the year that he was connected with the
Smithsonian Institution that our Association met in Washing-
ton, and for that meeting he prepared his first memoir on
meteorites, a subject to which he had become attracted by his
purchasing the collection belonging to Gerald Troost, of Nash-
ville. The study of these interesting bodies became thereafter
his favorite subject of investigation, and about forty of his
papers were devoted to them. He was active in collecting
specimens of American falls, and his collection, which con-
tained representatives of 250 falls, passed on his death to
Harvard University, swelling that collection until it became
the best in the country.

His study of meteorites led naturally to his devising im-
proved methods of analysis, especially of the silicates, and
while in Paris on one of his many visits there he became in-
terested in the discovery of new elements in the complex
mineral samarskite. He devoted much attention to the isola-
tion of its constituents, and at the St. Louis meeting of our
Association announced his discovery of what he believed to be
a new element, to which he gave the name of Mosandrum.
The announcement of the isolation of a new element by a past
president, gave to the chemical section in 1878, an impetus and
dignity that it has never relinquished. Dr. Smith was also
present at the Boston meeting, and it was about that time that
he further announced his discovery of certain rare earths, for
one of which, should it prove to be an element, he proposed
the name of Rogerum, in honor of our William B. Rogers.

He died in 1883, and there are still many of us who remem-
ber his interest and enthusiasm on all matters pertaining to
chemistry. Dr. Marvin has well said : "In him were united
great talents and profound knowledge, with such graces of

character as modest unselfishness and the most spotless integrity.''[1]

Dr. Smith served the Association of American Geologists and Naturalists as secretary in 1845, and was the general secretary of our Association at the Washington meeting in 1854.

Our Association has always been fortunate in its permanent secretaries. They have all been devoted to the interests of the organization, and two of them held office for many years. The first permanent secretary was Spencer F. Baird, who was chosen to that office at the Cincinnati meeting in 1851, and continued as such until 1854, when he was succeeded by Joseph Lovering, who then filled the place until 1873, when he in turn was succeeded by the present retiring president, Professor Putnam. Lovering's valuable services were recognized by his election to the presidency in 1872, and he presided over the meeting held in Portland a year later.

Lovering[2] was born in Charlestown, Massachusetts, now a portion of Boston, in 1813, and inherited a fondness for mathematics from his father, who was a surveyer by profession. He was fitted for college by the Rev. James Walker, and entered Harvard in the Sophomore year in   **LOVERING** the class of 1833, a class that included several members who were afterwards called to fill chairs in their *alma mater*, and one — Jeffries Wyman — who became a president of our Association. Lovering stood fourth in his class and he delivered the salutatory oration at the commencement exercises.

For a year after leaving Harvard he taught in Charlestown, but an inclination towards theology led him to enter the Harvard Divinity School, also at the same time devoting some attention to mathematical studies. It was probably that fact that led to his appointment as tutor in mathematics and physics in 1836, to fill the place made vacant by the illness of Prof. John Farrar, and thus his long connection with Harvard began which only terminated in his death, fifty-six years later.

In 1838 Professor Farrar retired from active duty and Lovering was made his successor in the Hollis chair of mathematics

1 Original Researches, p. 13.

2 See sketch in *Popular Science Monthly*, with an engraved portrait on wood. Vol xxxv, p. 600, September, 1889. Also see article in *Scientific American*, February 27, 1892, with a half-tone portrait.

and natural philosophy, which he then held for exactly fifty years, when he in his turn retired and was made emeritus. He was the first member of the Harvard faculty to fill a professorship for half a century, and his length of academic service was only exceeded by that of Henry Flynt, who was a tutor in Harvard early in its history. Lovering was also director of the Jefferson Physical Laboratory, holding that office during 1884-8, and he was a regent of Harvard during 1853-4 and again during 1857-70.

In the development of the Harvard astronomical observatory he took a prominent part. He was associated with Prof. William C. Bond in 1840, when with but few instruments and indifferent facilities, the beginning of the work in astronomy was made in the Davis House in Cambridge. It is from this small beginning that the present splendid observatory has grown. When Alexander von Humboldt induced the Royal Society of London to undertake the procuring of simultaneous observations on terrestrial magnetism in Great Britain and the colonies, the co-operation of the United States was sought, and one of the three stations in America was located in Cambridge where the taking of the observations was under the direction of Bond and Lovering. Several undergraduates of Harvard aided in the work, and among them was Benjamin A. Gould, who served the American Association as president in 1868.

The exacting duties of his work at Harvard and his own active interest in our Association left him but little time for scientific investigations. Still from 1867 till 1876 he had charge of the computations for determining transatlantic longitudes from telegraphic observations on cable lines, and under the direction of the U. S. Coast Survey, of which his colleague Benjamin Peirce was then superintendent, and the results of his work were given in volumes II and IX of the memoirs of the American Academy of Arts and Sciences. It was also to this source that he contributed in 1873 his great memoir on the aurora borealis. His shorter papers were more than one hundred in number and many of them appeared in our Proceedings. They testify to his unceasing activity as well as to his unusual ability. Mention should be made also of the fact that he was associated with Benjamin Peirce in the publi-
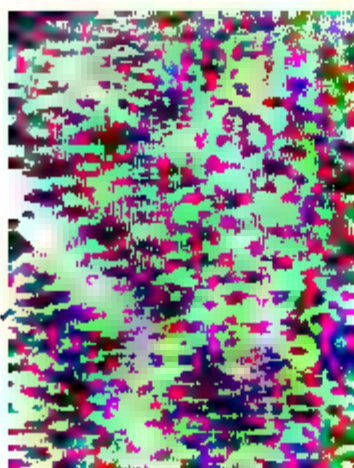
Asa Gray

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Hamilton Smith

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J. E. Hilgard

From "Appletons' Annual Cyclopædia."
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William B Rogers

From "Appletons' Cyclopædia of Amer. Biog."
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cation of the Cambridge Miscellany of Mathematics and Physics, to which he contributed articles on The Internal Equilibrium of Bodies ; The Application of Mathematical Analysis to Physical Research ; The Divisability of Matter, and similar subjects which attracted wide attention throughout this country and the scientific world.

Lovering was also an acceptable lecturer, and he gave nine courses, each of twelve lectures on subjects in astronomy and physics before the Lowell Institute in Boston.   He also gave shorter courses at the Smithsonian Institution in Washington, at the Peabody Institute in Baltimore, and at the Mechanics' Institute in Boston, and single lectures elsewhere.

It would be pleasant to review at length his work in connection with the American Association, but the memory of fifteen successful meetings and an equal number of volumes of Proceedings edited by him are all that need be mentioned. His interest in the American Academy of Arts and Sciences was also noteworthy.   He was its secretary during 1869–73 ; its vice-president during 1874–80, and its president during 1881– 1888.

At the time of his death this statement was made of him:

Harvard has met with a serious loss, as has the scientific world which benefited so much by his investigations. Behind him, however, he has left the results so well organized that the students of the present day can press forward to consummation of results which this teacher and exemplar did, and an incalculable amount to bring about work for the profession of which he had given the vitality of his mind and body.[1]

And so it was with the American Association, the magnificent pioneer work by Lovering made possible the wonderful successes by Putnam, during whose administration our Association reached its high tide of membership and attendance. We shall do well to place the name of Lovering high among those of the fathers of the Association.

The successful meeting in Portland was followed by the even more successful meeting at Hartford, Connecticut, which according to the permanent secretary, "was one that will make a special era in the history of the Association.[2] In attend-

[1] *Boston Herald.*

[2] Proceedings, American Association for the Advancement of Science, vol. XXIII, p. 150.

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ance it was one of the three largest meetings held, subsequent to their resumption in 1866. The presiding officer of the meeting was John Lawrence LeConte, of Philadelphia.

This distinguished entomologist[1] was born in New York City in 1825. He was of Huguenot ancestry, as is suggested by his name. The first of the family to come to this country was Guillaume LeConte, who settled in New Rochelle early in the eighteenth century. Among his descendents **LECONTE** were Lewis and John Eatton LeConte, both of whom achieved some prominence for their interest in science. The latter, Major John Eatton LeConte, entered the U. S. Topographical Engineers and was distinguished as a botanist and as an entomologist. His son is the subject of this sketch.

After finishing his collegiate education at Mount St. Mary's College, in Emmettsburg, Maryland, LeConte entered the College of Physicians and Surgeons in New York City and was graduated there in 1846. Possessed of independent means, he never took up the actual practice of medicine, but yielded to a fondness for natural history, inherited from his father, he devoted himself to travel, visiting many portions of the United States during the years between 1841 and 1851.

Says Scudder :

The subject of the faunal relations of animals was a favorite one with LeConte. He returned to it again and again ; he was the first to district much of the vast and then almost unexplored regions west of our prairie country.[2]

Later he traveled in countries outside of our own territory, and visited Honduras, Panama, Europe, Egypt, and Algiers, collecting valuable material, wherever he went, in the fauna of the country. These contributions to zoo-geography were, however, but accessions to his main work—the overflow of a mind charged with resources.

While still a student of medicine he published his first scientific paper, which contained descriptions of more than twenty species

[1] Biographical Memoirs of the National Academy of Sciences, vol. xi, p. 216. John Lawrence LeConte, by Samuel H. Scudder. This article, accompanied by a photo-gelatine portrait, appeared in the *Transactions of the American Entomological Society* for August, 1884. The *Popular Science Monthly*, vol. v, p. 620, September, 1874, contains a sketch with engraved portrait on wood.

[2] Biographical Memoirs, p. 272.

of Caribidae from the eastern United States. His preference for entomology continued throughout his life, and how industrious he was in that direction and what an influence he exerted on that branch of science, is shown by the statement that more than sixty monographic essays, some of them expanding to the form of a volume, and all of them after the first five years of work, direct and valuable contributions to the taxonomy of the order (Coleoptera) appeared from his pen.[1]

It may be added further that the order of coleoptera received his greatest attention. He described or at least named nearly 4800 nominal species of that group. His most important and extensive publications were devoted to them. These include Classification of the Coleoptera of North America, Part I, 1862; Part II, 1873; List of the Coleoptera of North America, 1866, and New Species of North American Coleoptera, Part I, 1866; Part II, 1873, all of which were published by the Smithsonian Institution.

The sketch by Scudder, from which so much of this material has been taken, contains the following appreciation of LeConte's work:

That LeConte was the greatest entomologist this country has yet produced is unquestionable. *Facile princips* will be the universal judgment both now and by posterity.[2]

Mention must be made of the fact that when the civil war broke out he entered the Union army as surgeon of volunteers and was afterward advanced to the office of medical inspector, with the rank of lieutenant colonel, which he retained until the end of the war. In this duty his fine organizing power and good sense showed themselves to excellent advantage. From 1878 till his death in 1883, he again served the government as chief clerk of the U. S. Mint in Philadelphia.

One of our past presidents, Lesley, who was his life-long friend, said of him :

Let the world reverence his memory as a discoverer, as a philosopher, as a genius.

For the meeting held in Detroit in 1875 Julius Erasmus Hilgard, of the U. S. Coast Survey, was chosen to preside, and thus for a third time in our history an officer of the U. S. Coast Survey was honored by an election to the highest office

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[1] Samuel H. Scudder. in Biographical Memoirs, p. 274.
[2] *Idem*, p. 280.

within the gift of our Association. Hilgard[1] was the eldest son of a distinguished Bavarian jurist and writer, and came with his father to this country in 1835. Although at that time only ten years of age, he had comple-

HILGARD

ted the third grade of the gymnasium in his native town of Zweibrucken, and his subsequent education was for the most part obtained from his father or self-acquired.

In 1843 he went to Philadelphia with a view to the study of engineering and practical employment. He was soon actively at work on one of the new railway lines then coming into existence. While so engaged he became acquainted with Alexander D. Bache, and in 1845 when Bache became superintendent of the Coast Survey, he offered young Hilgard a subordinate appointment in this service, which was promptly accepted with the statement that he preferred to ''do high work at low pay than low work at high pay.''[2]

At the close of the first year's field work it was said '' that the zeal and ability with which he has discharged these duties deserve mention here, as they have received it in the reports of the chief of his party.''[3] A year later it was said that he accomplished the work assigned to him ''under conditions of considerable hardship that might have excused him from keeping the field.''[3] His enthusiasm and interest in his work soon led to his recognition as one of the leading spirits on the survey. No task was too great for him to undertake, no duty too onerous for him to perform, and no problem too difficult for him to solve.

In the short time of fifteen years he rose from the lowest place in the survey to that of first assistant, which was second only to the office of superintendent. During the greater part of the civil war and until the death of Bache in 1867, the actual duties of the superintendent devolved on him. Peirce, who succeed Bache, said of this service.:

The distinguished ability with which this difficult service was discharged was manifest to all. He [Hilgard] has extended to me the bene-

[1] Biographical Memoirs of the National Academy, vol. iii, p. 327. Julius E. Hilgard, by Eugene W. Hilgard. See also sketch with engraved portrait on wood in *Popular Science Monthly*, vol. vii, p. 617, September, 1875, and Appleton's Annual Cyclopaedia for 1891, p. 628, with portrait.

[2] Biographical Memoirs, p. 330.

[3] *Idem*, p. 331.

fit of this experience liberally and loyally. While I willingly acknowl-
edge myself under deep and lasting obligations to him for the aid thus
rendered me, I can also testify that in all respects he has been equally true
to my predecessor, the greatness of whose reputation has not been dimin-
ished in his keeping.

Hilgard continued as assistant in charge of the office during
the superintendency of Peirce, and his successor, Patterson,
but in 1881 his services received their just reward by his ap-
pointment as superintendent of the Coast Survey, which place
he then held for four years. On the advent of a new adminis-
tration, after a faithful service of forty years, he was obliged
to resign. It is not pertinent to this address to discuss the
reasons that led to his resignation, but

There can be no two opinions upon the character and value of his life-
work in connection with the Coast Survey. He brought into that branch
of the public service a rare combination of culture, zeal, knowledge of the
world, and executive ability ; and no man living will claim to have done
more than he did for the character and efficiency of the survey.[1]

For many years, in addition to his other duties, Hilgard had
charge of the construction and verification of the standards of
weights and measures, and was for some time engaged in
preparing metric standards of great precision for distribution to
the several states. In this connection he was appointed a dele-
gate to the International Metric Commission that met in Paris
in 1872, and also a member of the executive committee of the
International Bureau of Weights and Measures. At the time
of its organization Hilgard was invited to become director of
this bureau, with its headquarters in Paris, but he declined.
Concerning this invitation the superintendent of the Coast Sur-
vey said that it marked

The sense held in this eminently scientific body of this special fitness
for organizing and conducting an institution so exacting in its scientific
demands and so novel in political inception.

The arduous and confining duties of his office in the Coast
Survey naturally limited his scientific work to the sphere em-
braced by his practical work, but he was also recognized "as an
active student in other brances of science, especially dynamics
and molecular physics."[2] Of such work, the most impor-
tant, however, was that connected with the magnetic survey

[1] *Science,* May 15. 1891.
[2] *Popular Science Monthly,* vol. VII, p. 618.

of the United States, which was carried on at the expense of the Bache fund, the direction of which was entrusted to Hilgard by the National Academy of Sciences.

His lectures on the Tides and Tidal Action in Harbors, delivered before the American Institute in New York, was regarded as remarkable for its lucid and terse exposition of principles without the aid of mathematical symbols. Later he delivered a course of twenty lectures before the students of the Johns Hopkins University on the subject of Extended Territorial Surveying, which was received with much appreciation.

His life-work, however, was in connection with the Coast Survey, and his relation to it will always be accepted as his greatest contribution to American science. From 1886 till his death, in 1891, he lived quietly in retirement, vainly endeavoring to regain the health and strength which he had sacrificed in the patriotic performance of his duty to the country of his adoption.

It was indeed a happy suggestion that led our Association in 1881 to recognize the life-long interest of William Barton Rogers in its welfare by electing him as the first of our honorary fellows. Rogers was the last presiding officer of the Association of American Geologists and Naturalists, and it was he who inducted to office William C Redfield, at the first meeting of the American Association in 1848. It is for this reason that his name stands first in the list of our presidents. This name also appears as the twenty-fifth on the list, for in 1875 he was honored by an election to the presidency and he presided over the meeting held in Buffalo in 1876.

It is not an easy matter to find a suitable designation for so versatile and accomplished a scientist as Rogers, for he was master of more subjects than one, and belonged to
ROGERS a period in the history of science, when teachers were students and authorities in several branches of learning. He was one of the four sons of Patrick K. Rogers, who, for a decade, was professor of natural philosophy and mathematics at William and Mary College, Virginia. William Barton[1] was born in Philadelphia in 1804, and followed his

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[1] There are many sketches of W. B. Rogers, among which are a notice of William Barton Rogers, founder of the Massachusetts Institute of Technology, by Josiah P. Cooke, in *Proceedings of the American Academy of Arts and Sciences*, vol. xviii.

parents to Williamsburg, in 1819. His early education was received from his father, and for a time he was a student of William and Mary. Later be became an assistant to his father, who wrote:

My second son, who is now in his twentieth year, and has a very extraordinary passion for physico-mathematical sciences.[1]

In the autumn of 1825, with his younger brother, Henry, he went to Baltimore, and there, for a time, pursued various vocations including that of scientific advisor to Isaac Tyson, the chemical manufacturer, but chiefly that of teacher in a school established by the two young men in Windsor. The pursuit of science was the aim of his ambition, and he was fortunate in securing an appointment, early in 1827, to deliver a course of lectures before the Maryland Institute. These were so successful that he gave a second course a year later. Concerning these Henry wrote to his father:

William is still able to command large and ever increasing classes. * * * I cannot refrain from expressing my surprise at William's great success, aided as he is by little more than the blackboard and chalk.[2]

Walker said of these lectures that he then

First displayed, upon an adequate field, that power of clear exposition and felicitous illustration which he possessed in a degree perhaps never excelled.[3]

In August, 1828, came the death of the elder Rogers, and two months later William was chosen his father's successor in the chair of natural philosophy and chemistry in William and Mary College, "and thence forward became, in a large measure, the head of the family."[4] For some years he continued in the active possession of that chair, also during part of the time temporarily filling the chair of mathematics. His professorial duties were naturally paramount, but it must be

p. 426; Memoirs of William Barton Rogers, 1804-1882, presented before the National Academy, by Francis A. Walker; The Brothers Rogers, read before the American Philosophical Society, by Dr. William S. W. Ruschenberger; and a memorial pamphlet issued by the Massachusetts Institute of Technology with a photo-gelatine portrait. There is also a sketch in the *Popular Science Monthly*, vol. IX, p. 606, September, 1876, with an engraved portrait on wood, where monthly there has been published a life and letters of William Barton Rogers edited by his wife, Emma Savage Rogers, in two volumes, Boston, 1896, that contains several portraits both of himself and of his brothers.

[1] Life and Letters, p. 26.

[2] *Idem*, p. 47.

[3] Biographical Memoirs, p. 3.

Life and Letters, p. 54.

noted that at that time he published a paper on Dew, and with his brother Henry one on the Voltaic Battery, both of which were subjects directly connected with his professorship.

Of subjects less directly associated with his college duties, to which he devoted much attention, were topics connected with geology. He wrote a series of articles on the Green Lands and Marls of Eastern Virginia, describing their value as fertilizers, and says Cooke:

Next we find the young professor going before the legislature of Virginia, and, while modestly presenting his own discoveries, making them the occasion for urging upon that body the importance of a systematic geological survey for developing the resources of the state.

The year 1835 saw the culmination of his ambition in that respect, for in March he was appointed director of the Geological Survey of Virginia.

As was anticipated, says Cooke:

The survey led to a large accumulation of material, and to numerous discoveries of great local importance. As this was one of the earliest geological surveys undertaken in the United States, its directors had, in great measure, to devise the methods and lay out the plans of investigation which have since become general. * * * [Also]·there are four or five general results of Professor Roger's geological work at this period, which have exerted a permanent influence in geological science.[1]

These general results included the study of the solvent action of water in various minerals and rocks; the demonstration that coal beds stand in close genetic relation to the amount of disturbance to which the inclosing strata have been submitted; the announcement and discussion of the wave theory of mountain chains; and the law of distribution of faults. In working out these subjects and in the presentation of papers discussing them he was associated largely with his brother Henry, who was at that time state geologist of Pennsylvania. It has been well said that "together they unfolded the historical geology of the great Appalachian chain."

Popular interest in the survey gradually dwindled and in the legislature decided opposition manifested itself until in 1841 its political enemies succeeded in preventing the passing of an appropriation and so the survey came to an end.

[1] Notice of William Barton Rogers, p. 429.

JOSEPH LOVERING.

It was also in the year 1835 that Rogers was chosen to the chair of natural philosophy and geology in the University of Virginia. Of his career, then President William L. Brown, of the Agricultural and Mechanical College of Alabama is quoted as saying :

I have seen his lecture hall so crowded with young men, eager to hear his eloquent presentation of the subject by the professor, whom they so greatly admired, that not even standing room could be found in the hall. All the aisles would be filled, and even the windows crowded from the outside with eager listeners. His manner of presenting the commonist subject in science—clothing his thoughts, as he always did, with a marvelous fluency and clearness of expression and beauty of diction unsurpassed—caused the warmest admiration, and often aroused the excitable nature of Southern spirit to the exhibition of enthusiastic demonstrations of approbations.

He resigned his chair in 1853 in order to devote more of his time to original investigation but the students never forget him and at the celebration of the semi-centennial of the University of Virginia in 1875, he received a perfect ovation. In the language of a contemporary Virginia newspaper :

The old students beheld him the same William B. Rogers who thirty-five years before had held them spellbound in his class of natural philosophy ; and as the great orator warmed up, then men forgot their age ; they were again young ; and showed their enthusiasm as wildly as when in days of yore enraptured by his eloquence, they made the lecture room of the university ring with their applause.[1]

Ever since boyhood it had been his cherished hope to work some day side by side with his brother Henry. Such an opportunity now presented itself. The younger man had settled in Boston some years previous, and released from the duties of his collegiate work, William B. Rogers gladly sought the congenial atmosphere of the northern city where it was possible to devote himself to original work. He associated himself with the American Academy of Arts and Sciences and the Boston Society of Natural History, taking an active part in the proceedings of both of these learned societies, in the latter of which he was in close communication with Agassiz, Wyman, and Jackson. At first during this period his papers dealt with matters of geology and paleontology but later he took up work in physics. No discussion of these publications is here

[1] Life and letters, vol. II, p. 325.

possible but that they were of high character is conceded. Concerning a paper discussing the phenomena of smoke rings and rotating rings in liquids which was published in 1858, Cooke said : "In this paper Professor Rogers anticipated some of the later results of Helmholtz and Sir William Thomson."[1]

The crowning and greatest work of Professor Roger's life was the founding of the Massachusett Institute of Technology. That achievement was so important in its results, so far-reaching in its prospects, and so complete in its details, that it overshadows all else.

In 1859 [says Walker], Professor Rogers, gathering around him a number of the first citizens of Boston, begun the public discussion of a scheme for technical education, to be associated on one side, with research and original investigation upon the largest scale, and on the other, with agencies for the popular diffusion of useful knowledge. So entirely unfamiliar to the public mind of the day was the idea of technological instruction, beyond the simplest requirements of civil engineering, that the Legislature of Massachusetts could not be brought to see the full merits of Professor Rogers' most comprehensive and as all now view it thoroughly practical plan, but enough was done by the Legislature during the few years following to secure the chartering, in 1862 and the inauguration in 1865 of the Massachusetts Institute of Technology of which Professor Rogers became the first president, devoting to it all the energy and enthusiasm of his impulsive nature and all the varied wealth of his accomplishments and acquirements. For the rest of his life this was chosen work.[2]

Rogers lived to transfer to a worthy successor the completed edifice,—well established and equipped—an enduring monument to the nobility of character and the consecration of talents. Honored and loved by his associates and students, he comes to be recognized as "founder and father perpetual, by a patent indefeasible."[3]

Of all the delightful memories of the Boston meeting in 1880 the meeting with Rogers is my pleasantest recollection. He was the central figure, losing no opportunity to make that meeting the greatest one in the history of our association. Never shall I forget when he rose

Tall in statue, with a figure of the type known to us through the pic-

[1] Cooke's Notice of Rogers, p. 433.
[2] Biographical Memoirs, p. 11.
[3] Cooke's Notice of Rogers, p. 427.

tures of Henry Clay; with a face destitute of all assumption or arro-
gance, was singularly commanding; with a voice whose compass and
quality were capable of producing at once the largest and the finest
effects of speech.[1]

and bade the Association welcome.   He said :

I thank my friends for the patience with which they have listened to
one who does not like to call himself an old man, but who feels something
of the spirit of the war-worn soldier, who likes at times to shoulder his
crutch and fight his battles over again.[2]

Two years later, at the same place, he rose to address the
graduating class of the Institute.

His voice was at first weak and faltering but, as was his wont, he gath-
ered inspiration from his theme, and for the moment his voice rang out
in its full volume and in those well-remembered, most thrilling tones ;
then, of a sudden, there was silence in the midst of speech ; that stately
figure suddenly drooped ; the fire died out of that eye, ever so quick to
kindle at noble thoughts, and, before one of his attentive listeners had
time to suspect the cause, he fell to the platform—instantly dead.   All his
life he had borne himself most faithfully and heroically, and he died as
so good a knight would surely have wished, in harness, at his post, and
in the very part and act of public duty.[3]

At the Buffalo meeting, in 1876, Simon Newcomb, "one
of the most celebrated astronomers of our time," was chosen
to preside over the Nashville meeting.   Newcomb still lives
and is our senior past president.   He marks the dividing line
between our earlier and later presidents.[4]

[1] Biographical Memoirs, by Walker. p. 5.
[2] Proceedings, American Association for the Advancement of Science, vol. XXIX,
1880, p. 739.
[3] Biographical Memoirs, by Walker, p. 13.
[4] Nature, vol. LX, p. 1. May 4, 1899.

PAPERS READ.

[TITLES AND ABSTRACTS.]

ONE ASPECT OF THE RELATION OF INDIAN CORN TO THE WHEAT PROB-
LEM. BY JOHN HYDE, Washington, D. C.

While over one-half of the total corn crop of the United States is pro-
duced in four States, there is a production of at least one-tenth of a bushel
to every acre of land surface and of at least five bushels per capita of pop-
ulation throughout the whole of that vast territory extending from the St.
Lawrence river to the Gulf of Mexico and from the Atlantic Ocean to
within sight of the Rocky Mountains. During the last ten years the pro-
duction of corn in the United States has averaged 1,850,000,000 bush-
els a year and there have been four crops that have each exceeded
2,000,000,000 bushels. Enormous, moreover, as is this production of corn,
it is unquestionably capable of indefinite expansion. Whatever contro-
versy there may be as to the wheat-producing potentiality of the United
States, there can be none as to the practically unlimited capacity of the
country for the production of corn.

The enormous production of corn and the wide distribution of its pro-
ductive area, together with the fact that hundreds of counties produce
largely of both corn and wheat and that on tens of thousands of farms the
two cereals grow side by side, have doubtless had much to do with the
extravagant ideas of certain writers as to the wheat-producing potentiality
of the country.

The first settlers in the new world found the natives eating bread of
maize. Maize, or Indian corn, is indigenous to the United States and its
production has gone on increasing, with but little effort on the part of the
agriculturist, throughout the entire period for which statistical records
are available. Wheat is not indigenous to the United States ; when first
introduced it did not take kindly to its new environment, and for many
years the colonists had great difficulty in growing sufficient for their own
requirements. It was a century and a half before there was available for
export a surplus equivalent to one-half bushel per head of the population
of the country. From one Federal Census to another, the per capita pro-
duction of corn has steadily gained upon the per capita production of
wheat. This has not been a matter of relative productiveness. To every
100 acres of wheat there was, from 1869 to 1878, an average of 177 acres
of corn ; from 1879 to 1888, 185 acres ; and from 1889 to 1898, 201 acres.

Corn is less liable to disease than any other cereal ; it has fewer insect
enemies than any other important product of the soil ; it has a greater
variety of important uses than any other, and the facility with which it
can be cultivated by hand adapts it to the more or less primitive methods

of agriculture still obtaining in certain sections of the country. It owes its commanding position, however, chiefly to its faculty of adapting itself to the most varied conditions of soil, climate, and cultivation.

Wheat is inferior to corn in all important uses, save only for human food. It does not adapt itself to its environment nearly so readily as corn, and the diseases from which it suffers and the insect enemies to which it falls a prey require the constant attention of the vegetable pathologist and the entomologist. Its cultivation has on various occasions been temporarily abandoned throughout extensive regions on account of the ravages of insects and disease.

The wheat-producing potentiality of the United States may remain to some extent a subject of controversy, but it is no argument in favor of the possibility of an indefinite extension of the area of production that the area within which corn can be successfully cultivated is almost coextensive with the country. Statistics show that even where the two crops grow side by side the conditions of temperature and moisture that are favorable to the one are frequently injurious to the other, and the fact that the United States may produce two billions of bushels of corn in a single crop is no evidence that it can produce even one billion bushels of wheat.

FEDERAL GUARANTIES FOR MAINTAINING REPUBLICAN GOVERNMENT
    IN THE STATES.   BY CORA AGNES BENNESON, A.M., LL.B., Cambridge, Mass.

The Constitution of the United States establishes, but does not define, a republican government. It guarantees to every State in the Union a republican form, but does not specify what authority shall exercise this power of guaranty. It agrees to protect each of the States against invasion, and on its application, against domestic violence, but does not name the means or agency for such protection. It, therefore, becomes interesting to inquire what is a republican form within the meaning of the Constitution and how the United States fulfils its guaranties.

When the Constitution was adopted, it guaranteed without change the governments then existing in the States. Their forms were, consequently, republican within the meaning of the Constitution. In all, the laws were made by representatives of the people, elected by a franchise for which the qualifications varied in the different States, but nowhere admitted the whole body of the people to suffrage. The States were at liberty to alter their forms of governments, under the restriction that they must not exchange republican for anti-republican constitutions.

The power to decide whether the form of government in a State is republican, fell upon Congress as a consequence of its power to admit new States. By requiring the Southern States to ratify the Fourteenth Amend-

ment before they could formally be restored to the Union, Congress practically implied that it would not recognize as republican any form of government that made class distinctions in legislation or excluded from the suffrage any man duly qualified. The Thirteenth Amendment had previously made slavery a bar to such recognition of a State.

When two governments dispute possession of the same State, as in Rhode Island in 1841–42 and in Louisiana and Arkansas in 1874, the established one must be recognized even though less republican than the other.[1] It is for Congress to decide which government is established and whether it is republican or not, but this decision it may delegate to the President, when it is necessary to enable him to determine whether an exigency has arisen for the employment of troops. The Courts will not inquire whether his decision is right.

The language of the Constitution does not fasten the guaranty upon any single department. It says merely, " The United States shall guarantee to every State in this Union a republican form." What department, then, is to enforce the provision, Congress, the Executive, or the Judiciary?

Two views have been advocated :

1st. That the power of guaranty rests in Congress.

2nd. That this power is concurrent in all of the government agents.

The exclusive claim for Congress is based on Art. I, Section 8 of the Constitution which authorizes that body "to make all laws which shall be necessary and proper for carrying into execution * all * powers vested by this Constitution in the government of the United States." Congress may, therefore, pass all laws that it considers necessary and proper for carrying into execution the guaranty clause. In the Rhode Island case before referred to it was held that it rested with Congress, to determine the means proper to fulfil this guaranty.

On the other hand, it is claimed that the language of the Constitution would have been " Congress shall guarantee," had this department alone been intended. In a case involving the status of Texas[2] after the Civil War, the Supreme Court of the United States, while affirming the Rhode Island decision, modified it by declaring that " the power to carry into effect the clause of guaranty is *primarily* a legislative power and resides in Congress." Practically the power is concurrent in all of the departments. Congress acts when legislation is required, as in the admission of new States and the reconstruction of the Southern States ; the Judiciary when the laws are obstructed; the Executive when military force is needed to repel invasion or suppress domestic violence.

In the last two cases the procedure differs. To repel invasion the President need not await a call for aid from the State invaded, since such invasion is a menace to the Republic ; he may send Federal troops and is empowered by Acts of Congress, also to call out the militia of any State or States for that purpose. In cases of domestic violence, it is the duty of the State to enforce its own laws, hence the President acts only on ap-

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[1] Luther v. Borden, 7 Howard 1.

[2] Texas v. White, 7 Wallace 700.

plication of its legislature, or executive, when its legislature cannot be convened. Thus the Federal government is deprived of every pretext for interfering with the internal affairs of any State except upon formal application.

If the insurrection in any way interrupts the operations of the National government, the President may send Federal troops and also militia to put an end to such interruption, without any call from the State. Sometimes the announcement that he has decided to act will be as effectual as if the troops had actually been called out. Federal forces were sent to suppress the railroad riots in Chicago in June and July, 1894, because the mails were obstructed, the processes of the Federal courts could not be executed by ordinary means and there was proof of conspiracy against commerce between the States. Militia from four States were sent by Washington to the Western counties of Pennsylvania, when they resisted the execution of the Federal Excise Law in 1794.

In both of the above instances, the President called the troops into action after legal notification by United States judicial officers. Most of the questions arising under the guaranty clause, however, call for political not judicial decision. When the greatest insurrection in our history broke out—the Rebellion—none of the governors or legislatures of the seceding States called for National interposition ; no Federal judge notified the President that the laws were obstructed. He took the initiative and issued a proclamation calling for 75,000 militia.

At the close of the Civil War, the Supreme Court held that the disorganized States were still entitled to the benefit of the Constitutional guaranty, which gave Congress authority to provide for the reestablishment of legal State governments in place of those overthrown. The Court did not consider it necessary to determine whether the measures taken in carrying out the guaranty were in all respects warranted by the Constitution. Federal intervention was constantly needed to sustain the reconstructed governments and many things were done in the emergency that are not now regarded by the best legal authorities as precedents for future action.

The language of the constitutional guaranty limits it to the States ; have the Territories any guaranty for a republican form of government ?

The Constitution confers upon Congress the power to dispose of and make all needful rules and regulations respecting the territory of the United States ; Congress may therefore give such territory any political form it pleases. So long, however, as a territory remains unrepublican it cannot be admitted to statehood.

Are the guaranties of individual civil rights in the Constitution a restraint upon Congressional legislation in the territory of the United States ? One guaranty clearly is,—the 13th Amendment,—declaring that " neither slavery nor involuntary servitude, except as a punishment for crime * * shall exist within the United States or any place subject to their jurisdiction." Whether the first ten amendments, the provisions for uniformity of duties, imposts and excises, of naturalization and bankruptcy laws, the apportionment of direct taxes according to population, trial by jury, the definition

of citizenship, with its attendant privileges and immunities, are restraints upon legislation outside of the States, are still undecided questions, whose answer depends largely upon whether the words " United States," are interpreted in these provisions to mean the Union of States only, the integral territory of the United States on our continent, or all territory in our possession. Upon this point there have been some dicta and judicial decisions but none based on a complete discussion and regarded as final. For most important purposes the Territories have been treated hitherto as parts of the United States, and no distinction has been made between the personal and property rights of their inhabitants and the inhabitants of States. The Federal Government, however, protects its Territories from invasion and domestic violence, as an incident of its ownership, not in consequence of any constitutional guaranty.

CALCULATIONS OF POPULATION IN JUNE, 1900. BY HENRY FARQUHAR, Department of Agriculture, Washington, D. C.

The problem discussed is : given the population by the census for successive decades, and given the immigration from one census date to the next, to determine the population probably to be returned next June. The " natural increase " for each decade, or the increase not directly due to immigration, is taken equal to p divided by $e + fp + gp^2$, where p is the population expressed in millions and decimals of a million, and e, f and g are positive constants found by calculation. The constant e alone would give a geometrical and f alone an arithmetical progression. The results of four calculations, depending on different initial assumptions, are tabulated below :

Designa-tion.	Values of Constants			Deduced population
	e	f	g	
A	2.862	.035	.00091	73.648
B	2.279	.086	.0	74.693
C	2.51	.073	.0	75.679
D	3.35	.0	.0012	74.466

In all these calculations the census population for 1870, known to be defective, was excluded. The immigration for the decade ending next June was estimated at 3,750,000.

Since the writer believed calculation C to be on some accounts preferable to the others, he concluded that the census would show probably more than 75,000,000 people, but not so many as 76,000,000.

[This paper is printed in full in the *National Geographic Magazine* for October, 1899.]

THE POWER OF THE CONSUMER, ECONOMICALLY CONSIDERED. BY MRS. FLORENCE KELLEY, Corresponding Secretary National Consumers League, New York City.

1. Everything is made for the consumer. If the whole body of consumers' should cease purchasing any given article, it must cease to be

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produced. The consumer is demand incarnate in the struggle of supply to meet demand. We speak of demand and supply as though they were superhuman entities. But if I am seeking a new suit for my schoolboy I represent demand, and if I am constrained by circumstances to buy a sweater-made suit, then demand does not control supply as the text-books teach, but the contrary is true.

2. However, supply does try to meet demand ; for this reason manufacturers study the market, *i. e.* they calculate, infer, guess from the action of the consumer to-day his action a year hence. Their success in business depends upon their ability either to guess correctly and so meet the existing demand ; or to persuade the public that it has an unsatisfied want, and so create a demand for their product. The failure of an enormous percentage of manufacturers shows how difficult is their task both of inference and persuasion. The recurring crises show that the difficulty is sometime unsuperable for the whole body of manufacturers at once.

3. In the effort to persuade the public, the manufacturer advertises. But the mass of advertising used by manufacturers can lay no claim to instructing the purchaser as to the article recommended, or as to the conditions under which it is produced. Advertising ordinarily aims to *stimulate* and *entice* purchasers, not instruct them. Indeed much of it, of which the patent medicine advertisement may serve as the type, is directed to the ignorance of the purchaser. Sometimes it seems to resemble the gong-beating of the midway, merely calling attention to the fact that buying is going on, and deafening and stupefying the passerby. It is only relatively recently that manufacturers have attempted to educate the public by exhibitions of their wares, asking comparisons of the relative merits of their exhibits ; and the latest advance in this direction is the Commercial Museum of Philadelphia, where permanent exhibits are promised for the instruction of buyers. What the enlightened purchaser wants is knowledge, as to the cost of the article, the ingredients of which it is made, and the conditions of manufacture. Of bread we wish to know not only whether the bakery is free from sewage and vermin and infectious disease, but whether the baker has his bedroom elsewhere ; how many hours he works and whether his wages enable him to keep his children in school. Such knowledge implies a degree of *power* which we have not yet fully attained.

4. Manufacturers who succeed in business do so by approximating to the wants of large bodies of purchasers. But the approximation is by no means always satisfactorily close. How few of our ready-bound books are just as we like to have them ; or of our ready-made shoes, or garments ! Baker's bread is a fair example of the articles made to suit the average buyer, and really suiting no one exactly.

5. While the manufacturer is guessing as to the intention of the purchaser, the purchaser is no less in the dark as to his goods, their origin, and history. A painful type of the powerless, unenlightened, unorganized consumer is the colony of Italian immigrants in any one of our great cities. They support at least one store for the sale of imported maccaroni,

vermicelli, sausage (bologna and other sorts), olive oil, Chianti wine, and Italian cheese and chestnuts. These articles are all excessively costly, owing to transportation and import duties; but the immigrants are accustomed to them, and they prefer to eat smaller quantities of these kinds of food, rather than a greater abundance of the foods which are cheaper. The sorrowful result is, that the importer buys the least possible amount of the Italian product, and uses that with all manner of native adulterations, as in the case of olive oil of which virtually none that is really pure is sold at retail. What the Italians get is the Italian label, and the familiar looking package with its contents tasting more or less as the Italian product tasted at home in Italy. What the actual ingredients may be, they know as little as we do, when we put our so-called butter, and our so-called maple syrup, on our hot cakes at a city hotel. The demand of the Italians for real native Italian products though large, persistent, and maintained at very serious sacrifice, is not an effective demand, for the immigrants have neither knowledge nor organization with which to enforce it.

6. Most of us are more or less nearly in the position of the Italian immigrants. In proportion as industry has gone out of the household and has become localized in certain cities and certain factories, the purchaser has lost acquaintance with the ingredients used, and with the conditions of manufacture. For certain great modern industries *men* have arranged tests of the product; and war-ships, locomotive engines, railway bridges, and electrical installations can all be tested, before the bills are paid. But for the industries which have gone *out of the home* nothing effective has yet been devised corresponding to these tests. The individual consumer after being *stimulated* and *enticed* by advertisements is incredibly ill prepared to select among the articles offered for purchase. What house-wife can detect, single-handed, the injurious chemicals in the milk, bread, or meat supply? What young girl knows that oil-boiled taffeta is more durable than ordinary silk at twice its price? Do we not all buy our wheels on the reputation of the firm, without knowledge of the qualities of the rubber, steel, iron, brass and leather of which they are made?

7. The community comes to the rescue of the buyer in some cases by employing a city chemist and bacteriologist to pass upon the qualities of milk, beer, etc.; and a few States have Boards of Officers who investigate rumors of adulterations of food products. But, in the absence of an organized and enlightened body of public opinion the story of honest officers hounded out of office, of weak officers bribed, and·of incompetents permanently retainéd, constitutes one of the blackest chapters of industrial history.

8. It is one of anomalies of the situation that, while the manufacturers spend millions in advertising for the purpose of *persuading* and enticing *purchasers*, the States and cities are spending hundreds and thousands in obtaining for the public a very slender modicum of trustworthy information concerning the very products in question.

9. The Boards of Health, the Bureaus of Labor Statistics, the Factory

Inspectors, all publish reports dealing with the subject of manufacture. All these institutions exist for the purpose of enlightening and instructing the public. But when I have carefully gone over all their reports, I am as much in danger as ever of buying glucose for sugar, acetic acid for vinegar, and paper in the soles of my shoes. To illustrate out of my own observation as Chief Inspector of Factories of Illinois: During the terrible epidemic of smallpox in Chicago in the summer of 1894, I was looking for a certain cigar-maker who was said to have smallpox in his family. Quite by accident I stumbled upon a tailor, newly moved into the house, and not yet registered either with my Department or with the Local Board of Health. In his shop there was a case of smallpox. In the shop there was, also, a very good fall overcoat in process of making, such as gentlemen paid $65.00 to $80.00 for in that year. In the collar of the coat was a hang-up-slip bearing the name of the leading merchant tailor of Helena, Montana. Now the tailor in Helena, Montana, had had in his large plate glass window samples of cloth from which the customer ordered his coat. The tailor had taken the necessary measurements, and had telegraphed the sample number of the cloth, together with the measurements, to the wholesaler in Chicago, whose agent he was. The customer was saved by the sheer accident of our stumbling on the coat in time to have it seized and burnt, from buying smallpox germs along with his expensive garment. Unfortunately, I do not, myself, see how our published record of that case can have served to save other purchasers from like danger of scarlet fever, measles, consumption, infectious sore eyes and other, equally, serious dangers. For the method of manufacture remains unchanged. Throughout that summer, the knee-pants made up in the infectious district were tagged "New York", though sewed in Chicago by Bohemians many of whom had never seen New York. Similar is the experience of the Factory Inspector in another State, who recently showed me the half page newspaper advertisement of a celebrated firm. " All our goods are factory made. We handle no tenement goods," and showed me at the same time his letter of warning to the firm not to remove a lot of girls' school cloaks found the day before in a tenement house room in which there was a case of scarlet fever. Said that Inspector " I can't stand on the street corner and wave this letter and that advertisement at the purchasing public ; and no one comes here to ask about these things but the people who know them already." Nor is there any punishment for such advertising.

10. The dissemination of information officially gathered has not, hitherto, been effective for lack of organized bodies of individual purchasers. In vain has the fact been published that the most fashionable chocolates of the day are made by Italian children of habits so filthy that the physicians who have been asked by the Factory Inspectors to examine these children as to their physical fitness to work, under the factory law, required the children to bathe, have their hair cut, and change their clothing before proceeding to the examination. The chocolates are as popular as ever. In vain is it pointed out that the bouillon placed on the market by the

famous Chicago packing firms is boiled in such close proximity to the fertilizer storage that the Factory Inspectors fall ill on the day following an inspection. The bouillon continues to be served at the luncheons of the socially ambitious. Such official publications are not read by the masses of purchasers.

11. Nor is this ignorance concerning the conditions of production regarded as a just cause for mortification and shame on the part of otherwise intelligent people. Ladies have been known to object to the inquiries made by the Consumers' League as to the condition of employment in the department stores, on the ancient ground that these concerned only the proprietors, and the purchasers were trespassing when they pushed their inquiries beyond the conventional conversation concerning prices carried on over the counter.

12. From all this it appears that as to any line of manufacture, the power of the purchaser is, to-day, virtually nil, in this country. In Great Britain, the great cooperative societies, by pooling their interests, are able to employ professional buyers who look after the qualities of their goods and the conditions under which they are produced. The effect of these great societies informing a stable demand for goods of excellent quality and approximately ascertained quantity is too well known to need reiteration here. In this country, there having been no such long, slow process of education of the purchaser, there is no such stable market for goods produced under right conditions.

13. Since the exodus of the industries from the home, the one great industrial function of women is the function of the purchaser. Not only all of the food used in private families, but a very large proportion of the books and furniture, and of the clothing for men and boys, as well as for women and girls, is manufactured expressly for the purpose of being sold to women. It is, therefore, very natural that the first effort to educate the purchasing public concerning the power of the purchaser should have been made by women, among women, and in behalf of women and children. Having proved somewhat successful in that limited field, it is now extending among people of both sexes and all ages, and is asking the cooperation of learned bodies such as this.

14. The first effort in this country to organize miscellaneous purchasers, and ascertain their power, was made in New York City, nine years ago, by two ladies, Mrs. Charles Russell Lowell and Mrs. Frederick Nathan. That effort was directed exclusively to the *consciences* of purchasers. The ladies selected the two stores in which the conditions of work of women and children were the most humane ; and they set forth the good points of these stores, as their standard. They wrote to 1400 storekeepers in New York City inquiring whether they wished to arrange their work in conformity with the Standard, and have their names included in the White List. Of the 1400 storekeepers *two* replied.

From that modest beginning has grown the present White List of the New York City Consumers' League, with its thirty-nine leading department stores, for the two ladies proceeded to organize their friends and

enlist others ; to circulate the White List and the Standard upon which it is based ; to bring their growing membership to the attention of storekeepers ; and to educate the public by meetings, and through the press, concerning the powers of the purchaser. Finding that many stores failed to respond to their efforts, they procured the enactment of the Mercantile Employees Law which places stores, in New York State, under requirements similar to those which the factory law imposes upon manufacturers; and they urge shoppers to observe scrupulously whether seats are furnished for saleswomen, and to see that cash girls are not undersized, that the store closes early, etc.; so that the enforcement of the law by the officials is reinforced by the purchasing public.

15. The Consumers' League of New York has found imitators in Pennsylvania and Illinois. In Massachusetts, the projectors of a State Consumers' League discovered some three years ago, that the State Laws as administered by the State Factory Inspectors had already brought about conditions as favorable in Boston as those sought by the New York Consumers' League, while the great department stores of Boston had voluntarily adopted the eight hours' day.

16. The people of Massachusetts, however, have their own grievance as to the lack of power of the purchaser and they formed their Consumers' League on a broader basis than the New York one to deal with a different though a kindred evil. In Massachusetts, under their rigorous sweatshop law, enforced by their faithful and competent Inspectors, sweatshops have been driven out of the State. But, under the constitution of the United States, Massachusetts cannot prohibit the importation of goods made in other States, and Massachusetts manufacturers, therefore, find themselves undersold by the product of the sweatshops of New York ; and Massachusetts purchasers find themselves in danger of buying goods made in infectious shops, in New York, at prices as high as though these dangerous goods were made in the cleanest of the Massachusetts factories, for there is no way of distinguishing the goods after they reach the counter of the retail store.

17. Since the same evil, manufacture under the sweating system, exists in other States, the four Leagues of Massachusetts, New York, Pennsylvania, and Illinois have now formed a National Federation of Consumers' Leagues for the purpose of organizing purchasers with a view to improving the conditions of manufacture in the garment-trades. By way of ascertaining, by practical experiment, the extent of the power of the purchaser, in this field, the National Consumers' League has adopted a label to be placed on garments made under conditions satisfactory to the League. It has selected, as the first branch of manufacture for its experiment, women's white muslin underwear. Since May 1st, a number of factories in Maine, Massachusetts, Rhode Island, New York, and New Jersey have been visited and investigated with a view to placing the Consumers' League label on their product. Some of these have proved to be "model" factories ; others are vastly better than the sweatshops. It is the aim of the League to cooperate with these and other humane and enlightened man-

ufacturers ; to contract with them to maintain certain conditions in their factories and then encourage purchasers to give the preference to the product of those factories, by buying goods carrying the label of the Consumers' League.

18. The present requirements of the League in regard to factories using the label are four; to-wit, all goods must be made on the premises (to prevent sweating):

All the requirements of the State Factory Law must be complied with; no overtime must be worked ; children under sixteen years of age must not be employed.

19. Employers have been found who are not only willing to comply with these requirements, but have been doing so for years, with no recognition from the purchasing public of their humane arrangements as contrasted with the shops of the sweaters.

20. Several questions, however, arise as to the probable success or failure of this extension of the work of the Consumers' League.

First, and most important, is the question " How can purchasers feel assured of the validity of such a guaranty, given by a volunteer body without any legal sanction ? "

Second : " How can a sufficient body of purchasers be listed to make it worth the while of inhumane manufacturers to come to the standard of the League? "

Third : " What is the position of the League with regard to that vital consideration of the employee, the payment of wages?"

21. Answer 1. The League has a contract under which it can sue for selling under false pretenses any manufacturer who uses its label otherwise than as agreed. It also seeks the cooperation of the Factory Inspectors, the local labor organizations, and educational institutions, and avails itself of all accessible means of information concerning the good faith of the manufacturer. Finally, no factory is accredited until it has been inspected by the League's own Inspector who was, four years, Chief Inspector of factories of Illinois.

22. Answer 2. The Consumers' League seeks the cooperation of the Women's Clubs (The General Federation of Women's Clubs having undertaken to devote an entire session to this subject at its biennial meeting in June 1899) ; and of all educational institutions. The Association of Collegiate Alumni has arranged for its next meeting a discussion of " The Teaching of Economics in the Colleges Included in the Association with Especial Reference to the Theory of Consumption," having been asked by the Consumers' League to take up this important subject.

23. The Universities have been the centers of the most active organization; thus Professor Seligman, of Columbia, Professors Ashley and Taussig, of Harvard, and Henderson, of Chicago, advocate the work of the League; and its President is Mr. John Graham Brooks, of Cambridge. One of the most active centers for promoting organization is Wellesley College. Among thinking people, the response to the suggestion of the League as to the power of the consumer is most encouragingly prompt and persist-

ent. Manufacturers show their confidence in the value of the effort to promote organization and stable and enlightened demand, by offering to bear the expense of printing the labels, attaching them to their wares and defending them in court in case of any forgery or infringement.

24. Answer 3. The Consumers' League cannot, at present, touch the question of wages for two reasons : Its constituency is not yet sufficiently tested in practice to justify it in dealing with the manufacturer at his most vitally sensitive spot ; it cannot, in its first season, assure him of a sufficient addition to his previous list of customers, to make this demand. Second, the League has not yet enlisted a sufficiently large number of employees to enable it to gather the required data for setting up a wage-standard under the piece-work system. It appears that the regulation of wages will have to be accomplished by the manufacturer, the employee, and the consumer jointly, and cannot be done by the consumer at present without a broader basis of information.

25. In general, the power and usefulness of the Consumers' League will depend upon the intelligence and character of its local centers, and the degree of cooperation which these succeed in enlisting in the general public. At present the Consumers' League points out that the consumers, even when unorganized, have power to put an end to the production of any given article by refraining from purchasing it ; to promote the production of a given article by demanding it ; when organized even very partially, consumers can decide, within certain limits, the conditions under which the desired articles shall be produced. Consumers have, however, hitherto, done none of these things in an orderly and enlightened way except so far as cooperative buying has been introduced and the prevention of adulteration has been promoted by consumers. The power of the purchaser which is potentially very great, becomes great in practice, as shown by getting his own way, only in proportion as he unites, organizes, gets into communication directly with the manufacturer, informs himself concerning the conditions of manufacture and substitutes knowledge for the stimulation and the persuasion of the advertising seller.

THE BASIS OF WAR AND PEACE. BY M. A. CLANCY, Washington, D. C.

The recent meeting at the Hague of the so-called peace conference suggests some thoughts on peace and war. Whatever may be the result of that conference, it is doubtful if a general disarmament will be accomplished.

War is not confined to man. Nature, which formed him from herself, imparted her essences, powers, and tendencies, to be exhibited in a new and special manner. Peace, likewise, is derived from nature. War is essentially destructive ; peace essentially constructive. Natural forces represent these two opposite tendencies. Extremes of heat and cold are destructive to life, while the mingling of these extremes in due propor-

tion is constructive. Similar results arise from drouth and humidity. Every natural department reveals this doubleness of function, and illustrates a universal law, wherein opposite tendencies and forces are combined to produce the actual.

Man's evolution discloses the transition from ignorance to knowledge. Ignorance, coupled with desire, leads to frictions, antagonisms, rebellions, wars ; increasing knowledge diminishes these ; but, unless absolute knowledge could be attained, their entire abolition could not be assured. The practical result is such combination of ignorance and knowledge, or such degree of knowledge, as mental growth and natural environment produce. Egotistic desires, first in order and predominance, constitute the basis of war, as shown by savage and barbarous tribes. As altruistic sentiments prevail, war lessens in intensity ; and in civilization great modifications of warfare take place, due partly to these sentiments and partly to improvements in weapons and means of carrying it on.

Theology lends its aid both to war and peace. Early theological teachings inculcate sentiments of vengeance, as witness the commands of Jehovah of the Hebrews. Grecian mythology also exhibits similar traits. Later teachings, especially those attributed to Christ and Socrates, enjoin sentiments of love and brotherhood. Christ commands his followers to " love their enemies.'' These teachings, however, are not addressed, in preponderance, to the thinking, but rather to the feeling side of the mind ; they are commands to children, to be obeyed, not addressed to the rational faculty, to be considered and understood.

The perception of law is the foundation of genuine peace. The understanding that there is no room in nature for the exercise of arbitrary power, dispels the illusions of the gods, and furnishes the basis of satisfaction with things as they are and must be ; thus influencing men's minds in a manner far transcending all former conceptions. Man thus becomes a law unto himself, a link in the chain of Universal Law.

Ameliorations of the horrors of war are seen in saving the lives of prisoners, treatment of the wounded enemy, respecting property, not contraband of war, etc. But that there will ever come a time when the forces that make for war will absolutely overcome those which make for peace, or *vice versa*, is a dream never to be realized. As long as human passion and human ignorance shall endure, so long will there be occasion for more or less of difference, error, strife, war. There is here an "irrepressible conflict between opposing and enduring forces," and the total destruction of either is an impossibility.

THE INCREASE IN THE MEDIAN AGE OF THE POPULATION OF THE UNITED STATES SINCE 1850. BY MANSFIELD MERRIMAN, Lehigh University, South Bethlehem, Pa.

The median age is defined as that age which separates the population into two classes of equal size, one-half being under and the other half

over that age. The method of finding the median age from the census statistics is explained.

It is shown that the median age of the white population of the United States increased from 18.6 years in 1850 to 21.9 years in 1890, and that it will probably be 22.9 years in 1900. For the colored population the median age is now about 17.6 years. For the entire population the median age was 18.3 years in 1850, 18.9 years in 1860, 19.6 years in 1870, 20.4 years in 1880, and 21.4 years in 1890, the present rate of increase being about one year in each decade.

The following table is presented as giving the results of the investigations of the author, the figures for 1900 being estimated by using the rate of increase from 1880 to 1890 :

MEDIAN AGES FOR THE UNITED STATES.

Year.	Whites.	Native Whites.	Colored.	All Classes.
1850	18.6	....	16.5	18.3
1860	19.3	....	16.5	18.9
1870	19 9	16.2	17.7	19.6
1880	20.9	17.8	17.5	20.4
1890	21.9	18.9	17.6	21.4
1900	(22.9)	(20.0)	(17.6)	(22.4)

TRUSTS : A STUDY IN INDUSTRIAL EVOLUTION. BY H. T. NEWCOMB, Department of Agriculture, Washington, D. C.

The classical political economy of England is a study of industrial relations under a régime of free competition and, as first formulated, was largely a protest against legislative interference in the conduct of industry. As early legislation of this kind was addressed directly to the individual, there has come to be a not unnatural confusion of thought by which free competition is made to seem almost synonymous with political liberty. Thus American and English devotion to liberty have been made the basis of a prejudice favorable to competition in trade which is rarely subjected to critical examination. The aphorism that " competition is the life of trade " is accepted by the general public as an economic axiom and is made the major premise of a syllogism of which the vital importance of perpetually unrestricted competition is an inevitable conclusion. To public opinion thus summarized, the industrial progress of the nineteenth century has consisted very largely of a succession of exceedingly disquieting phenomena which are suggested by the terms factory system, trades union, corporation, consolidation, combination, railway pool, and the more recent term of indefinite significance, "trust."

Though the system of household industry in vogue during the eighteenth century seemed to offer an almost perfect field for free competition, it was never entirely freed from the control of custom, and it cannot be said that the realm of competition has ever been coextensive with the domain of industry. It furnished the groundwork for the classical system of political economy, but even while the latter was being formulated, Hargreaves and Arkwright were at work upon the inventions which, with that of Dr. Edward Cartwright, a few years later, have revolutionized the business of manufacturing cotton. These inventions are prototypes of modern mechanical progress, and out of them the factory system has developed. Up to the period that may be roughly marked off as having begun with 1870, there had been a great development of the factory system. Factories had increased in size, railways had developed and had been consolidated into extensive systems, labor had formed itself into trade associations, corporations were numerous and controlled great wealth, but all of these institutions were separate. The new period is characterized by more comprehensive combinations, by railway pools, unions containing workmen of all or many trades, of manufacturing establishments combined in "trusts." The latter name survives though the legal entity to which it was properly applied has become obsolete. The early trusts were created through the transfer of the property of various establishments to trustees who issued trust certificates to the original owners and sometimes gave mortgages also upon the property placed in trust by each. These organizations met great popular opposition and were found exceedingly vulnerable to legislative attacks. They lasted long enough to impress upon capitalists the advantages of consolidation and were then superceded by gigantic corporations organized to take over the property formerly belonging to separate individuals, firms, or smaller corporations. Popular opinion attaches to these new corporations the incident of monopoly, but in so doing is never quite accurate. For example, we hear much of trusts in the iron industry, but 25 per cent. of the furnaces are operated independently of each other, while the remaining 75 per cent. is controlled by fourteen separate organizations, all bitterly antagonistic to each other.

The phenomena suggested have been the product of natural causes, are all closely related, and the latest was indicated by the earliest. To explain these relations it is necessary to examine the nature of industrial competition, and especially to consider the modifications which it has undergone as the result of the introduction of machinery. A theoretically simple classification of industries depends upon whether they conform (a) to the law of diminishing returns, (b) to the law of constant returns, or (c) to the law of increasing returns. The facts which determine the place of a particular industry, under this classification, belong to its technique, but the results are of the utmost importance among the postulates of economics. Technical evidence, available to the economist, indicates that all industries pass, or may pass, through successive states in which they conform respectively to

the law of increasing, of constant, and of diminishing returns. When an industry is in the first state, each establishment tends to increase its output, and as such increase, other things being equal, reduces prices, the process must eventually crowd out those who produce under the least favorable conditions. The position of marginal producers is therefore unstable and such industries tend toward monopolization in the hands of the single producer whose cost per unit of production is lowest. Industries which now conform to the law of constant or to that of diminishing returns, have reached these states, in nearly all cases, before the establishments have grown to the size which would permit them to supply the entire market. Machinery has extended the application of the law of increasing returns and thus tended to remove industries from the competitive field.

With regard to industries conforming to the law of constant or to that of diminishing returns, it is apparent that they may be divided into supplementary classes based upon the amount of capital required. Where establishments have grown to considerable size before entering these classes there is invested in each a large amount of fixed capital. If, in such cases, the marginal value of a product is reduced for any cause, the former marginal producer may be unable to divert his capital to any other industry. If so, he will be obliged to choose between an immediate total loss or a diminishing or even negative rate of interest. This suggests an advantage which an establishment controlling a large aggregate of capital inevitably possesses over a competitor of seriously inferior financial strength. The former may temporarily depress prices until the resources of the rival are exhausted, and this course may be pursued so far as to establish for a time a practical monopoly. There is no incentive for the creation of a new establishment which will have to submit to competition upon such terms, and this differential in favor of the old establishment is therefore a barrier to the investment of new capital, possibly to the utilization of improved machinery, or the inauguration of more economical methods, or more efficient organization. The effect of the introduction of machinery in this connection is apparent. It is also evident that it has reduced the remaining class, the only one in which competition can be permanent and beneficial, to a few industries, of which agriculture is the most prominent example at the present time.

Capitalistic combinations are, therefore, performing a useful public service, though there remains an inquiry as to the distribution of the beneficial results. Such combinations should tend to decrease prices to consumers, to increase the demand for raw material and therefore the price, to augment wages and ameliorate the condition of employees, and there is some evidence that they have done so. It is clear that while they tend to the centralization of industrial control, they tend also to the diffusion of industrial ownership. The latter tendency would be more notable, were it not for the popular attacks upon these combinations and the danger of adverse legislation.

The process of industrial development involves continuous readjustment and rearrangement. Capitalistic combinations have naturally tended to expedite the necessary changes. The temporary evil results have therefore been more acute in their consequences, though they are doubtless sooner over than they would be under a simpler competitive system.

It should be remembered that these combinations are in the form of business corporations and that this institution is the creature of legislation. As such it is a proper subject of statistical investigation and should be subjected to the utmost publicity that is consistent with the orderly transaction of legitimate business. At present it is impossible to say much more than this except that investigation should precede action and that legislation should never be based upon unconsidered generalizations which may hinder much that is good in order to destroy evils for which there is a much simpler remedy. It is reasonable to hope that society may find a way to enjoy the most economical productive methods and at the same time to have satisfactory and equitable distribution of the products of industry.

MORAL TENDENCIES OF EXISTING SOCIAL CONDITIONS. BY REV. WASHINGTON GLADDEN, Columbus, Ohio.

The paper undertakes to estimate the moral tendencies of the system of industry now prevailing, to find out what kind of men it is producing.

IMPORTANCE AND DIFFICULTY OF THE UNDERTAKING.

The system of industry now existing is based on free contract and competition. Slavery and feudalism have been succeeded by a competitive régime. Herculean efforts are now in progress to subvert this system, and substitute for it something very different, but these efforts, as yet are only partly successful. It is not easy to separate the injuries to character which arise from the abuses of the system, from those which have resulted from its normal operation. Still it is probable that the worst abuses of the system spring from its essential principles. The monopolist only seizes and uses the power at which competition bids him grasp. We must take the whole industrial world together, that which agrees with the theory of free contract and that which conflicts with it, and find out, if we can, how the characters of men are affected by their contact with the work and the traffic of the present day.

I. Gains Which We Seem To Be Making.

1. In common honesty. Probably less of cheating and fraud in ordinary business than there was fifty years ago. Some deduction to be made from this gain on the side of the buyers. More reckless running in debt than formerly. On the whole, however, a gain on the score of honesty in trade.

2. In the development of the fiduciary virtues. Under the present system many men are trusted with properties and enterprises in which they have no proprietary interest ; and while we have embezzlers and defaulters the number is surprisingly few. Great multitudes are being educated to fidelity.

3. The system enforces a valuable training in cooperation. We are learning that the highest welfare of each depends on the service of all.

II. Losses Which We Seem To Be Incurring.

1. The weakening of self reliance and initiative. Lessening opportunities of independent enterprise. We are learning to cooperate, but we are losing the power to set ourselves at work. Seasons of depression are inevitable accompaniments of the present system; and in each of these many hundreds of thousands learn the lesson of dependence.

2. The lowering of moral standards through an exaggerated popular estimate of the importance of material wealth. The competitive régime, with wealth as the prize of the competition, inevitably produces this result. Out of this grows a haughty and overbearing temper in those who have won great wealth, and anger and resentment on the part of those less fortunate ; the social bond is sorely restrained.

Politics is corrupted through bribery, education by the worship of wealth and religion by the awe of the rich pew-holder. All this is the natural fruit of the popular belief that money is the principal thing.

Against these tendencies there is strong resistance. The moral ideals were never more clearly held or more bravely maintained than now by teachers, preachers, writers, and artists. Will the moral forces be adequate to hold in check the demoralizing tendencies, or will there be need of changes in the existing forms of industrial and social organization ? Probably some such changes will be found needful. Doubtless the world will never be reformed by changes in the machinery of society, but doubtless the world will never be reformed without such changes. We could not change a despotism into a democracy without changes of political forms. All political and social ideas must have their own appropriate forms of expression. And if, in our industrial relations, the spirit of fellowship and cooperation is to be cultivated, and the spirit of strife and competition is to be repressed, it is needful that the forms of industrial fellowship and cooperation be substituted so far as we wisely may, for the forms of strife and competition. These changes must be gradual and cautious, but they must come : the new wine must have the new wine-skins.

SOME THERMAL DETERMINATIONS IN THE HEATING AND VENTILA-
TING OF BUILDINGS. BY GILBERT BURNET MORRISON, Principal of
Manual Training High School, Kansas City, Mo.

A description of apparatus used in a series of experiments to deter-
mine the relative economy of different methods of heating buildings,
and the position and approximate quantity of carbon dioxide in occu-
pied rooms. The apparatus consists of a model house with adjustment
devices for admitting the air, applying the heat, and testing the air.
The heat is furnished by electricity, and by furnace-heated air. The
results of ten experiments illustrating current methods are described
and tabulated ; and comparison of the consumption of fuel in different
methods is shown in a plotted diagram. Two other illustrations of
apparatus are made from etchings on zinc plates. The paper closes
with a comparison between the gravital and mechanical methods of
ventilating buildings showing that the gravital methods now in use
are comparatively ineffectual and wasteful, while mechanical methods,
when the exhaust steam from the engine is used for heating, costs very
little.

ON THE INJURY RESULTING TO THE EYES AND VISION OF SCHOOL CHIL-
DREN FROM WANT OF PROPER REGULATION OF LIGHT IN THE SCHOOL-
ROOM ; ALSO PROPOSED METHOD OF ESTIMATING THE RELATIVE
INTENSITY OF LIGHT FOR SANITARY PURPOSES. BY ARCHILAUS G.
FIELD, Des Moines, Iowa.

Myopia, the typical defect of vision in school children. Elongation of
eyeball the typical pathological condition of uncomplicated myopia.
Physiological action of the accommodation apparatus produces tempor-
ary elongation of eyeball, and therefore myopia, in near vision and also
in bright light, the former by changing the conjugate foci of the lens
the latter by reducing the size of the pupil. Persistence of the abnor-
mal condition exhausts the elasticity of the function of accommoda-
tion, and the temporary pathological condition becomes chronic, per-
manent, and incurable.

Prevention of long-distance use of eyes in schoolroom, and syste-
matic modulation of the light, to be indicated by exposure of slips of
sensitized paper a definite time and compared with a scale representing
known values, the amount of discoloration indicating the intensity of
the light.

Impartial provision for each scholar requires approximately even
and uniform lighting which cannot be provided with windows on one
side only, or one side and one end of a room.

In the photographs is shown the effect of light on sensitized paper
exposed thirty minutes on a bright day, in rooms in which the pro-
portion of glass area to floor space varied from 1 to 2 to 1 to 20, the

extent of browning representing the intensity of light in the several rooms.

SCIENCE AND ART IN THEIR RELATION TO SOCIAL DEVELOPMENT. BY JOHN S. CLARK, Boston, Mass.

The old ideal of scientific research was that of study for its own sake or for knowledge in the abstract. To-day, scientific research aims through knowledge at the solution of practical problems,—the attainment of practical ends. Man is taking his place as a conscious, willing factor in evolutionary progress. In the laboratory of the chemist, the biologist, the electrician, even the psychologist, analytic research is the basis of synthetic, creative action.

The product of creative activity is what we call art. Art is a world of man's own creation, a world where $2+2=$ more than 4. The old distinction between industrial art, aiming primarily at utility and fine art aiming primarily at beauty, is purely theoretic. In experimental fact no sharply dividing line can be drawn between the two; they melt each into the other.

The relation between science and art is that of mutual dependence. The dependence of art upon science is a generally accepted fact; the complimentary fact is sometimes overlooked. The problem of housing and handling a great public library makes new demands in every direction,—on the engineer, the bacteriologist, the mathematician, the chemist, the cataloguist, the electrician and physicist. It is the thought or idea that sets the pace. Gradually awakening desires for the spiritual content of fine art are spurring the physical and chemical sciences to their utmost in multiplying and improving processes of picture reproduction. It is in this direction of newly opening spiritual needs in man's nature that social development comes. The savage is conscious of little more than animal desires; civilization is measurable by the growth of conscious need in man's spiritual nature.

Again, the relation of science and art on the one hand to civilization or social development on the other hand is also one of mutual dependence. Urban centralization is both a cause and an effect. Closer contact of life with life develops forms of spiritual consciousness and spiritual need previously unknown.

It is not a mere coincidence that the last century, full of scientific research, should be also the mother of landscape painting and of the poetry of nature. The fact is significant. Fine art in one or another form has thus far been the one lasting product of a people's life. The labor expended for purely material, physical needs is for the most part untraceable in any tangible shape. Studying the life of any race or any period to know its ultimate object and product, what it was all

1—20   1~15

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I -4   I -2

DIAGRAMMATIC REPRESENTATION OF THE INTENSITY
OF LIGHT IN A SCHOOLROOM WITH WINDOWS
ONLY UPON ONE SIDE.

for, we find the answer in the fine art of that race or that period. The great economic problem of our own times lies here. The development of spiritual needs and desires can have no conceivable limit. In the realm of spiritual creation, in art, man is destined to find the fullest room for the exercise of his broadest powers, toward the highest ideals.

SOME NEW ASPECTS OF EDUCATIONAL THOUGHT. BY THOMAS M. BAL-LIET, Superintendent of Schools, Springfield, Mass.

Educational thought has been profoundly modified by a number of more or less recent discoveries in biology, physiology and psychology ; it has gained a new outlook upon many of its problems from the study of them in the light of evolution; and it has been materially broadened by the recognition of industrial, economic and social conditions as determinative factors in the solution of educational questions.

The discovery of the functions of the brain and their localization has thrown considerable light on several problems of the highest importance. It has shown that the human brain is an organ whose functions are highly differentiated and that education must take account of this fact. It has demonstrated that there is a sensory area, a motor area, and an area, not yet definitely made out, which forms the physical basis of the association processes. In the sensory area there is a visual center, an auditory center, and a center for each of the other senses. Each center can be stimulated only by stimuli from its own sense, and sensation as a conscious process takes place in these centers. These centers after repeated stimulation acquire the power of functioning in the absence of direct stimuli from the senses, and thus form the basis of memory and imagination. Jastrow has shown that persons who become blind before the age of five years never dream of things visible, but that persons who become blinded after the age of seven in all cases do. This would seem to indicate that at least seven years of direct stimulation through the senses are necessary to develop the power in the visual cells of the brain to act automatically, or in response to inward stimuli, as they do in dreams and in the processes of memory and imagination.

This emphasizes in the first place, to speak briefly, the urgent necessity of systematic sense training in the early years of childhood. It emphasizes objective methods of instruction over against reading and the study of books wherever possible in primary education.

In the second place, it emphasizes the fact that we have memories and imaginations rather than a memory and an imagination, each center having its own memory and imagination, from which it follows that there is no one study in the curriculum whose chief function is to

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develop the memory or to cultivate the imagination, but that the processes of memory and of imagination are to be developed, in so far as they are the direct outgrowths of sense activity, by appeals to all the various sense centers, especially to those of sight, hearing and the motor sense.

The discovery of special language centers has thrown light on the problem of teaching a child to speak, read, and write. It has been proved that the phenomena of aphasia are perceptible to the trained eye of the expert in the normal development of the speech functions in the case of every child. In the adult aphasic these phenomena are due to injury or disease, in virtue of which the speech mechanism is no longer able to function ; in the case of the child they are due to the fact that this same mechanism is not yet able to function because of lack of development.

Besides these sensory areas, there is a distinct area in each half of the brain whose function is to contract the muscles and to coordinate the resulting movements. This motor area is subdivided, there being distinct centers for the movements of the different parts of the body, such as the head, arms, trunk and legs. The arm area is again subdivided, and there are distinct portions of it which move the fingers, the wrist, the forearm and the upper arm.

The bearing of these discoveries on the general problem of motor education, including both physical culture and manual training, is of far-reaching importance.

In the first place, they emphasize the effect of physical training on the brain as a physical organ. These motor brain centers, like other bodily organs, need exercise for their growth. The only possible way to stimulate them is by means of voluntary muscular contractions. Some form of physical training is therefore necessary for the healthy growth of these centers. It has been found that where a limb has been amputated in childhood the cells in the brain center controlling that limb remained in quite a rudimentary condition, whilst amputations in case of adults have not had the same effect. In the case of bed-ridden invalids who had not used their muscles for many years, the entire motor area has been found more or less atrophied and water-logged. In the second place, psychologists have pretty clearly demonstrated that from muscles, ligaments and the inner surfaces of joints, there emanate sense impressions to the brain which are just as real as the impression received through the eye, the ear, and the other senses. In a word, there is such a thing as a motor sense which is stimulated by means of muscular contractions and the effects of the resulting movements. These motor sensations develop into corresponding ideas; and, like the latter, are recalled in memory and become integral parts of the higher thought processes. These motor ideas are, however, not merely the result of muscular movements, but when developed they control muscular movements, and constitute an inte-

gral part of what we denominate "manual skill." Such manual skill therefore resides not in the hand but in the brain and the mind, and the fact that it cannot be developed in the idiot is due not to any imperfections of his hand but to the defective character of his brain.

Moreover one of the more recent generalizations of psychology is that all thought is essentially motor, that the incipient contraction of some muscle or group of muscles is always involved in thinking; or, in other words, that thinking is only repressed action, and that the legitimate end of knowing is doing. It is also a part of the current theory of the will that it has its roots in motor ideas, developed at first through involuntary muscular contractions, and that in the utter absence of such ideas the power of volition could never develop. A high authority, to emphasize this thought, asserts that if a new-born child should be so bandaged as to make all muscular movements impossible and should be kept in that condition, he would inevitably become an idiot. There is considerable ground for the apparently extreme theoretical proposition that without a muscular system neither intelligence nor will could ever have evolved in the race. Whether these strong statements be strictly borne out by facts or not, there is abundant evidence to show that muscular movements are most intimately related to thought and will.

Moreover, there is a similar close relation between the powers of inhibiting muscular movements and that of self control in morals. The man who has acquired the power of controlling his muscles is by that fact rendered more capable of governing his passions and desires. The problems of physical and moral education present themselves to the educator in a new light when viewed from the standpoint which these truths furnish. Physical education and manual training reach deeper than muscles and nerves, and they cannot be simply added to the curriculum in a mechanical way but must form an organic part of it. Ethical training cannot be added in a similar mechanical way but must come largely as the result of right training in all other directions. In a word, the organic unity of physical, intellectual, and moral education is recognized to-day as it never was before.

The study of bodily growth of children has thrown some important side lights upon educational problems, and has modified educational theories. It has been found that growth is periodic. There is a period of moderately rapid growth, from about the sixth to the eighth year; this is followed by a period of very slow growth from the ninth to the twelfth or thirteenth year, and this again is succeeded by a period of very rapid growth from about the thirteenth to the sixteenth year, this last period coming somewhat earlier in girls than in boys. It has been found that growth throughout the year is also periodic. During the spring and earlier summer months children seem to grow most in height, during the late summer and autumn months in weight, and during the cold winter months very little in either respect. It is ob-

vious that these facts have a direct bearing on the amount of intellectual work that may safely be demanded of children at different periods of their lives and at different seasons of the year. The mechanical uniformity of our school programs in this respect is unquestionably working injury in our schools.

Growth, moreover, is not uniform throughout the whole body at any one time ; or, in other words, not all the bodily organs grow at an equal rate at the same time. Growth seems to focus now upon one organ and then upon another. Thus there is a very rapid period of growth in case of the brain up to the end of the eighth year, when that organ reaches almost its maximum weight and size, although there is a slight growth of an exceedingly important kind which continues to the age of thirty, and in some cases, as recent investigations show, extends even into the forties. There is a period when the heart grows rapidly, and physical exercises which are very wholesome after this period is passed and the heart has acquired its full power, may be injurious before that time.

The period when an organ can be most effectively developed, and, if need be, modified so as to counteract the effects of unfortunate heredity, is the period of its most rapid growth. From this it follows that the problem of physical training is a more complex one than is now generally recognized. It must have in mind not merely the body as an organism growing as a whole, but it must have due regard to the periods of growth for the various organs.

Speaking generally, the different parts of the body mature in the order in which they were originally evolved, those which are biologically the oldest maturing first and those which are biologically the most recent, last. The older and fundamental muscles of the trunk and limbs and their nerve centers develop accordingly earlier than the accessory muscles of the hand and the vocal organs and their nerve centers which are the products of quite recent evolution. It is of capital importance in education that fundamental organs be developed before accessories ; and a reversal of this order, which is not uncommon even in good schools to-day, proves often exceedingly injurious. The attempt to develop prematurely the muscles of the vocal organs by difficult vocal gymnastics and injudicious phonic exercises is a case in point. It has been shown that stuttering is largely a school-bred disease, and it makes its appearance during the period when children are learning to read. The fundamental muscles of the chest and diaphragm controlling voice and breathing should be developed before the accessories controlling articulation and utterance. In like manner the fundamental muscles of the arm should be developed before the accessory muscles of the hand and fingers and their nerve centers. The fine sewing, weaving, and other manual exercises in our kindergartens, is a violation of this principle which has just recently received recognition by teachers and school officials ; and the time will come when it will be recognized that writing should be taught at a later period than is done to-day even in the best schools.

It is a well-known biological fact that parts of organisms most recently evolved are, even at their full maturity, least stable, can stand the least wear and tear, and are most readily injured by over-exertion or disease. This explains why the use of the sewing needle and the pen may develop nervous troubles, whilst a much more prolonged use of the hammer, the ax, or the crowbar would not. In the light of this fact we can appreciate the exceedingly injurious effect of excessive fatigue upon these recent, accessory parts, incident to our premature attempts at developing them.

It is a well-known fact that in the human body are found many rudimentary organs which were functionally active in some of the lower animals but are of no use to man. Of these the pineal gland and the vermiform appendix are perhaps the most familiar examples. Biologists have discovered over one hundred and thirty of them.

It is but reasonable to expect that in the evolution of the human mind there should have been transmitted by heredity from our savage ancestors, and even from animal life below man, instincts, feelings, and impulses, which were once necessary for the preservation of the organism but are no longer in the same sense necessary to man.

One of the most familiar of these instincts is that of killing for mere sport in the form of hunting and fishing. This instinct has been inherited from our savage forefathers with whom fishing and hunting were necessary means of maintaining life. The instinct is no longer necessary but remains with us as a rudimentary organ. There is the fighting instinct which was developed by the conditions of prehistoric life. Besides instincts of this character, there are the feelings towards natural objects and the phenomena of nature which were developed through the first contact of the savage mind with its environment. There is the instinctive fear of darkness which is so strong in all children; there is the fear of thunder and lightning and storm which remains as an instinctive fear with many persons through life; there is the fear of animals and of strangers so common in children; and there is the love of the woods and of individual trees. These and many others, are no doubt echoes of a distant past, psychic fossils which testify to a life and to conditions of life in a remote antiquity.

Now, it is remote ancestral instincts, feelings and impulses such as these that conscious life is largely composed of during its earliest years in the individual child, and it is these elements or ingredients of mind which must be studied if we wish to understand child-life and lead it aright either in school or out. What is needed, therefore, to-day above everything else in elementary education is a pedagogy of the instincts.

One of the problems of moral education is to transform these primitive instincts into higher ethical impulses, and so convert them into elements of character. Arrested on their original low plane, they produce abnormal man,—the pauper, the tramp, and the criminal. Sin in all its forms is but little else than an arrest of ancestral instincts on their primitive plane. The fighting instinct, so strong in every boy at a certain age, if arrested, gives us the brute; if crushed, the coward; transformed it becomes an

integral part of moral courage and of that grit and determination which make for strength and force of character. The instinct of acquisition developed through the struggle of life, if arrested, gives us the miser and the thief ; if crushed, the pauper ; but if transformed, produces the man of thrift, industry, enterprise, and self respect.

Educational thought has been greatly modified in recent years by a recognition of the fact that industrial, economic, and social conditions demand that it shall connect more closely with life than has been the case in the past. Hitherto we have treated educational problems too much as if education were a process completed in school, and we have largely ignored the fact that it continues through life. The school should connect so closely with life that the education which it begins may be continued afterwards by the environments. The demand for a more practical education than either the secondary schools or the colleges have given in the past, does not spring wholly from mercenary motives but is prompted by a true educational instinct, and to appreciate it fully we must revise our theories of educational values. Hitherto in all discussions of such values no account has been taken of the question as to whether the education of the school is afterwards continued by the conditions of life, and yet this is a fundamental question. A study may have a very high educational value as a mere mental discipline while it is being taught in school, but may not connect directly with anything which comes in the after life of the student, and so have, on the whole, a lower educational value than another study which furnishes less discipline in school but connects with the after life of the student in such a way that the development which it begins is continued through life.

Viewed from this standpoint, the educational value of the traditional classical curriculum is distinctly lower and that of the curriculum of the technical school distinctly higher than is commonly assumed. Whilst the rapid growth of technical education is no doubt largely due to the demands of our industries, it comes also as a response to a real educational need. It is a fatal error to assume, as is not infrequently done, that because a particular study has a high practical value it must necessarily have a low disciplinary value. The truth is quite the opposite ; viewed by its effect on the entire life of the individual, the most practical study, as a general rule, has the highest educational value. Educational thought has distinctly broadened by a recognition, even if only partial, of this fact.

It has been the accepted theory for many years that the aim of education is the harmonious development of all the powers of mind and body. If the term ''harmonious development'' has any definite meaning in this connection it must mean symmetrical development. Under the influence of this theory, courses of study were constructed which were supposed to bring about such development, at least as far as the mind was concerned, and these courses were rigidly enforced upon all alike. Beautiful as this ideal at first sight seems, it is essentially false, and it would have been a misfortune if it had ever been realized. Men are not

created symmetrical, and education can not make them so. It is a waste of effort and of time to attempt to develop in a student powers which he does not possess on the assumption that he must be developed harmoniously. The ideal of the future will be, not to develop human beings symmetrically, but to educate each one as broadly as possible along the lines of his greatest strength. It is the first duty of education to discover the special talent of the student, and the second to train it.

Individual taste and ability will be respected more and more in the education of the future, and the elective system which has produced such admirable results in our best colleges must be introduced with proper safe-guards into our secondary schools.

What is needed to-day in higher education is earlier and broader specialization. The student should be allowed to arrange his studies earlier with special reference to his life work, and he ought to have more time and better opportunity to study this work in a broad way. He is obliged to spend too many years in so-called "liberal" study and has too little time to devote to the broad study of his profession ; in a word, our university courses are too short and the course required of a student in the college is too long.

I believe that the first step which ought to be taken to remedy this condition of things, is to extend the work of the public high school and thus to pave the way for the ultimate dropping out of the college. Good public high schools give a training to-day, in their four years' courses, which is quite equivalent to the training our best colleges were able to give forty years ago ; the quality of the teaching is higher and the facilities for teaching the natural sciences are better than was the case in our colleges at that period.

Our larger colleges are now doing strictly college work during the first two years and attempt to do university work during the last two. The work of the first two years of the college could be done more successfully in our high schools, where the numbers are small per teacher, and effective classroom instruction is possible.

Such university work as is attempted during the last two years of the college course could be done far more effectively by the universities which possess the extensive and costly equipments necessary for thorough university work and which our colleges do not have. Through such a readjustment at least one year could be saved without injury to the cause of higher education.

The effect of such a change would be a happy one on the cause of secondary education. It would bring the advantages of what would be virtually a college education to the doors of the masses of the people. It would reduce the cost of preparation for the university and the professional school at least one-third by enabling students to make this preparation at small expense near their homes. It would as a consequence open the way to the university for many capable young men and women to whom it is now closed.

The saving to the community in which a high school of the grade indicated should be located, would be very considerable, and it is this fact which would probably win popular favor for the plan. I happen to know of a city with a population of about 57,000, from which there are at present some eighty students in attendance in the first two classes of various colleges. The average expense annually to their parents is not less than $500 and is probably more. The total cost of educating these eighty students is therefore at the lowest estimate $40,000 per year. In this same city the addition to the teaching corps of the high school required to teach these students quite as effectively as they are taught in the colleges, could be provided for at most $10,000. The cost of board for these eighty students at their homes during the period of the college year would probably not exceed $9,000, thus making the total cost of educating them at home $19,000 and saving annually $21,000. I believe the time is not far distant when the best public high schools in this country will occupy a position in our educational system not very unlike that of the Gymnasium and Real Gymnasium in Germany and that of the Lycee in France, and when our stronger colleges will be converted into universities and our weaker colleges into academies of high school rank.

THE MANUAL ELEMENT IN EDUCATION. BY CALVIN M. WOODWARD, Washington University, St. Louis, Mo.

The introduction of the manual element began in America simultaneously in the kindergarten and in the chemical laboratory some forty years ago.

In the primary classes it has hardly got beyond the kindergarten room and the program laid down by Froebel. As by an edict the manual element ceases the moment the child takes the alphabet and the First Reader in his hand.

On the upper side, under the stimulus of the Morrill Act, endowing the A. and M. Colleges, some crude efforts at combining theory and practice were begun between 1865 and 1876.

Under the lead of the Massachusetts Institute of Technology, the Physical Laboratory came slowly into being in the 70's. The manifest superiority of laboratory physics over book physics as a means and method of intellectual culture caused it to spread into all progressive communities. At present science laboratories in the branches of chemistry, physics, zoology, and botany are wanting only in the strongholds of mediaevalism.

School work-shops came into being in the 70's. At first the school was brought into the shop; later the shop was brought into the school. At first commercial methods were employed and shop-teaching was fragmen-

tary and incidental ; finally scientific teaching took possession of the shop and the commercial idea has nearly disappeared.

To John D. Runkle, of Boston, and the Manual Training School of St. Louis, jointly, the true manual training idea of educational tool-work is due. From those two centers of influence it has spread throughout the world.

THE PERSONAL EQUATION AS A PSYCHOLOGICAL FACTOR. BY LAURA OSBORNE TALBOTT, Washington, D. C.

Since science is but in its beginnings, we may not go far astray in striving to call attention to a subject which would naturally seem to interest every reflective mind.

Fifty years ago many well-known facts, now attested by eminent medical authority have shown the effect of climate and atmosphere upon the human organism.

The use of an instrument to record the personal equation of exact mathematicians and astronomers, is not of ancient date, and new methods are constantly being introduced into our public institutions to detect the psychological peculiarities of the individual.

Ought we not to be able to extend the study of these natural phenomena to such an extent that more definite knowledge may be obtained of the relative effect of one human organism upon another.

To an earnest student of child-life, it must be apparent that there are certain elements of a young child's nature which are beyond the comprehension of investigators at the present time. The relations of the child to its fellow beings are of this class.

No subject seems more difficult to understand than the complex relations of human beings towards each other.

Religion has occupied a part of that field, which is not within the province of the spiritual nature of man, as our physical and intellectual relations with other human organisms are the result of mechanical activities.

If more careful discrimination were made in the different elements of a child's nature, we might better understand the cause of excessive anger of one child toward another, or the reason for jealousy, envy, and the like. Children are but the epitome of the race, the characteristics are so strongly marked in childhood that it would seem not such a difficult task to investigate the interchange of personal relations existing between a child and its human neighbors.

The physical foundation for the activities known as the sensibilities, are, as we all know, being made the subject of research work by our great nerve experts ; at the same time, it may not be amiss to call attention to the condition of the public mind in regard to the subject which is at all times coming to the front under so many different guises. No doubt the

whole attitude of the human mind, as it now looks upon the relations of one human organism toward another, would have to be changed, in order to be made to realize the possibility of relegating jealousy, envy, and like passions to the domain of the animal part of our nature, and that observations and experiments might greatly facilitate the advent of discoveries that might prove that our sensibilities are the result of overstrained mechanical activities of our being.

Many systems of education have sought to deal with this study of the relation of one human organism with another, but they come and go, without much benefit to mankind ; is it not because they claim too much, and do not discriminate between the sentimental and the mechanical parts of our nature ? Surely as population is increasing to such a great extent and competition threatens to injure society, may we not hope that some of our students may be able to find the thread in this snarl of nerves, brain, and passions that now prevent a clear knowledge of the relations of one human organism to another, at least may we not arouse an interest in the subject, that may lead governors of childhood to inquire if they are not themselves at fault, in not giving more attention to the personal equation of the individual ? No one appreciates more than the writer the difficulties in the way of a comprehension of this study of the reflex relations existing between human beings. Since it is one of vital importance to mankind, it is to be hoped that the writer may be patiently borne with on account of an honest desire to arouse discussion for further information upon this subject.

"THE SPOILS SYSTEM, IN THEORY AND PRACTICE." BY H. T. NEW-COMB, Washington, D. C.

THE SCOPE AND METHOD OF POSITIVE EDUCATION. BY MRS. DANIEL FOLKMAR, Brussels, Belgium.

Education must rest upon a scientific, a positive basis. The claims of psychology and child-study as being necessary data to education are recognized, but it is not sufficiently recognized that physiology, hygiene, neurology, sociology, in fact all the anthropological sciences, and a goodly number of non-anthropological sciences contribute equally important data. The present is a time of materialistic, of deterministic philosophy, a time when all hypotheses must rest upon a study of the facts.

Biologically, education is a modification—adaptation, specialization, and habituation of a living organism by any influence whatever:

Sociologically, human education is the conscious purposeful effort of one or more individuals of the human species to modify—adapt, specialize, and habituate other individuals of the species. In either case the ethical end is the evolution, adaptation, survivance of the species, the complete living and specialization of the individual.

The science of education is a positive study of all educational phenomena of the past and of the present, and a generalization of the laws governing such phenomena. Its general method is that of induction. Instead, however, of attempting to observe at first hand all educational phenomena, it accepts the inductive inferences of other sciences. Beginning with these as assumptions, they are related to and verified by specializations, and generalizations reached as to educational phenomena in particular.

The art of education includes (1) prevision, the foreseeing of ultimate and relative, remote and immediate ends of education and the means which will be utilized in their realization: and (2) practice, the carrying out of such a series of procedure as will result in a desired end. Prevision weighs all forces of the present and of the past affecting in any way educational phenomena and computes their resultant to take place in the future. The methods of all practice, therefore of educational practice from that of the philosopher down to the grade teacher, comprises the following steps: (1) an idea of an end or purpose; (2) the desire to reach the end; (3) the idea of means necessary to reach it (materials, tools, processes); (4) the selection of the means; (5) the completed volition or action; and (6) habituation resulting from repetitions of desiring, choosing, acting.

EXECUTIVE PROCEEDINGS.

REPORT OF THE GENERAL SECRETARY.

The forty-eighth meeting of the American Association for the Advancement of Science was called to order on Monday, August 21, 1899, at ten o'clock A. M., in the chapel of the Ohio State University, Columbus, O., by Prof. Frederic W. Putnam, of Cambridge, Mass., the retiring president. Prof. Putnam, with a few remarks, introduced the president of the meeting, Dr. Edward Orton, of Ohio State University.

PRESIDENT ORTON, after taking the Chair, introduced General Axline, who welcomed the Association on behalf of the State of Ohio, and Judge M. B. Earnhart, representing the city of Columbus.

The president next introduced Dr. WILLIAM O. THOMPSON, president of the Ohio State University, who spoke in substance, as follows :

I am the only one of my class here this morning. The other gentlemen who have been delighted to accord you a welcome have had the opportunity of standing to represent distinguished gentlemen. I have the honor of representing what we hope the future will always say was a distinguished institution. I am glad to stand here this morning as the president of the Ohio State University and recall to your mind a few familiar truths concerning the progress of education, which truths themselves form the best possible welcome that a company of men engaged in scientific education and its advancement could have as a welcome. Already reference has been made to the ordinance of 1787 as the foundation of the education of this Northwest Territory ; that this is the first state carved out of that splendid territory ; and also that that ordinance and the legislation that proceeded from it was the basis of the great public school education of this Northwest Territory ; nor need I remind you that that public spirit in public education was the real foundation on which the State University was founded ; nor need I remind you that the State University movement in this great Northwest Territory has been the most characteristic movement in education to us in the west in the last twenty-five years. But the Ohio State University is the last in this great list—practically the last. While we are open-handed and our means so largely dedicated to education, Ohio, with its usual conservative spirit, has been the last one to begin the work of a great state university. We stand upon the very border ; and I recall to your minds—those from the east—that beyond us and west of us are a dozen or fifteen state universities older than we are, greater than we are, with more pretentious buildings, with larger revenues;

and we in the west are just entering upon the beginning of the great things that stand before us in the way of higher education. I may recall to your minds that this great state university movement is the people's movement; and in all these great state universities that lie from here westward there is nothing more characteristic than the sincere effort to bring education to the people of a kind and character that shall fit men not for the professions but for the full, rounded and complete life in this newer civilization. It is the chief glory of these educational institutions, in my judgment, to make a sincere effort to bring to the youth of this great western country an education that shall enable them to live, and to live well, in what we believe to be the finest civilization of the world. We are proud and happy to stand here at this open gate of the great west, in one of her newer state universities, and welcome to our borders a company of distinguished gentlemen and ladies representing the main lines of thought in science and scientific education. If there is any place in the wide world where you ought to be assured of a welcome it is in a state university ; and I speak not simply for the Ohio State University, but for all of them to the west of me, when I say that any one of these institutions would welcome you as heartily to-day as do we in this university. We are hand-in-hand in this great movement for a broad, liberal, and complete education. We are glad you are here and we want to open our doors and our hearts—we want to open the university campus and desire that you should take everything before you and make yourself thoroughly at home here and there about the campus and through these buildings ; and if there is anything we can do, we assure you now it will be done most cheerfully, to make your stay pleasant, welcome, and profitable. We are delighted to see you here ; and we specially ask you to remember that the State University campus, with all on it or about it, is a home of welcome to every lover of science and to every lover of education who appears upon the grounds this week. (Applause.)

PRESIDENT ORTON, in behalf of the Association, responded as follows:

General Axline, Judge Earnhart, President Thompson, and Citizens: In behalf of the American Association for the Advancement of Science I return you our cordial thanks for the welcome you have this day given us to the capitol of Ohio. We note with pleasure all that you say of the state, the city, and the university. We are sure that you have not exaggerated the delights of this noble section of the Mississippi Valley which, all things considered, in location, in climate, in its agricultural and mineral resources we take to be, as the great Frenchman, De Tocqueville, declared three-quarters of a century ago that it was "The noblest dwelling place that God has fitted up for the occupation of the human race."

The principal office of such a reception as this, I take it, is to set both parties at their ease—to put host and guest on good terms with each other. You have spoken for the host and we accept all that you claim, all that you say, for the city and the state at its face value. We count ourselves happy to be here; we are glad to enjoy for a few days your hospitality. We hope and expect that the Columbus meeting of the Association will

prove a memorable one, and that it will shine in our annals, if not by the announcement of great discoveries in the heavens or the earth, still by the inspiration that it will give to multitudes of workers in all the varied fields of science.

Let me say a word for the guests on this occasion. I shall ask you to accept, without any discount, all that we say about ourselves, all that we claim for our work, in the same generous spirit that we have already shown toward you. (Applause.) People come to be on good terms with each other more usually, perhaps, if they are first on good terms with themselves. As our host you have shown a good measure of self-complacency; and you will not expect that we fall behind in this virtue. Well then, I may say for the American Association, that it is well worthy of the hospitality which you proffer. It deserves all the honor that Columbus or any other community in the country can pay to it. No organization ever visits an American city that has a better claim on the protection and respect of all its people. In the first place you can hardly expect to entertain an organization of larger range, so far, at least, as its name is concerned. It is the "American" Association—transcends not only all state limits but national boundaries as well. An organization that represents the United States takes in a quite respectable part of the land areas of the globe; but this is not merely a United States organization—it especially includes that potent and ambitious neighbor of ours to the northward that owns more than three million square miles, a full half (but we are not prepared to say the best half) of the North American continent. The Association always counts with all confidence on a Canadian contingent. You can hear this afternoon an address from the honored Canadian vice-president of one of our sections. Our name is broad enough to include also our neighbors to the southward—Mexico, and the Central American states; but these countries have so far devoted so much of their time and force to military science in the practical way that they have not had much enthusiasm left for the cultivation of the other branches of science in which our Association is especially interested.

But there are American associations and American associations : they are not all alike. They are devoted to various interests. Some of them in spite of their broad name have but a comparatively narrow field. For example, there is an American Numismatic Society; there is an American Stockbreeders' Association, an American Strawboard Company ; but this is the American Association for the Advancement of Science. As I have said, no organization can well have a broader name ; and when we come to its subject-matter,—the field in which it works,—certainly no organization can claim wider interest and greater importance. The American Association for the Advancement of Science,—have you considered what this name implies? We are coming to the close of the nineteenth century, which has been well styled the century of science. Alfred R. Wallace has recently published a careful inventory of the discoveries and inventions to which the progress of the race is mainly due; and he divides them into two groups, the first embracing all the epoch-making inven-

tions achieved by man previous to the present century and the second taking in the discoveries and inventions of equal value that have had their origin in our own century. In the first list he found but fifteen items of the highest rank. The claims even of this number may not all be un-questioned ; but he puts into this list the following : alphabetic writing, the Arabic notation, which have been the two great engines of knowledge and discovery. Their inventors are unknown, lost in the dim twilight of prehistoric times. Coming, after a vast interval, to the fourteenth cen-tury, A.D., we find the mariner's compass ; in the fifteenth the printing press,—both of which, unquestionably, are of the same character and rank as alphabetic writing. From the sixteenth century we get no visible in-vention or discovery but witnessed an amazing movement of the human mind which in good time gave rise to the great catalogue of advances made in the seventeenth century, the most prolific of all the centuries an-tecedent to our own. The seventeenth century enumerated the invention of the telescope and, though not of equal rank, the barometer, the ther-mometer and, in still another field, the invention of the differential cal-culus, the all-important discovery of gravitation, the law of planetary mo-tion, the circulation of the blood, the measurement of the velocity of light. During the eighteenth century we realized the more important of the earlier steps in the evolution of the steam engine, the foundation of both modern chemistry and electrical science. This completes Wal-lace's list. Counting all these inventions and discoveries as separate we get sixteen ; but Wallace places the barometer and thermometer under one number and makes a total of fifteen,—fifteen inventions and discov-eries up to the year 1800.

What is there to be added to the list? Some would urge that general discoveries should be included here ; but this claim Wallace would indig-nantly refuse to accept. In making any such list as this it is evident that the personal equation of the author undoubtedly needs to be recognized ; and different orders of arrangement, even if the achievements were the same, would be recognized by different students. At any rate something like this is the list of what the race has gained in science since it first came to itself, up to the year 1800. The greatest steps have certainly been counted. And now what is there recorded since 1800? How does the nineteenth century compare with its predecessors? A brief examina-tion will show us that in scientific discovery and progress it is not to be compared with any single century but rather with all past time. It far outweighs the total progress of the race from the beginning up to 1800. Counting on the same basis as that which he had previously employed, Wallace found twenty-four discoveries and inventions of the first class that have had their origin in the nineteenth century against fifteen or sixteen (already enumerated) for all past time. This is not the proper occasion to review, or compare, or state in order the several elements of this glorious list ; but let me simply recall to your minds a few of them. Of the same rank with Newton's theory of gravitation which comes from the seventeenth century stands out the doctrine of the correlation and

conservation of forces of our own century,—certainly one of the widest and most far-reaching generalizations that the mind of man has yet reached. Against Kepler's laws from the seventeenth century we can set the nebular theory of the nineteenth. The telescope of the seventeenth is overbalanced by the spectroscope of the nineteenth. If the first reveals to us myriads of suns, otherwise unknown, scattered through the fields of space, the second tells us what substances compose these suns, —what maintain their distant fires and, most wonderful of all, the state of aggregation of matter, whether solid or gaseous, the direction and the rate in which each is moving. Up to a recent date the motion in the line of sight of fifty-six stars had been determined in the discoveries of the world. I am proud to say that five of the fifty-six,—one-eleventh of the whole number,—were determined in McMillin Observatory, which is included on our premises. Harvey's memorable discovery of the seventeenth century finds a full equivalent in the germ theory of disease of the nineteenth. The mariner's compass of the fourteenth century yields first place to the electric telegraph of the nineteenth. The barometer and thermometer of the seventeenth are certainly less wonderful, though perhaps not less useful or serviceable than the telephone or the phonograph and the Roentgen rays. I need not pursue the comparison exhaustively ; but in addition to the advances of knowledge enumerated the great doctrine of organic evolution, supported especially by the recapitulation theory in embryology, finds nothing to class with it in broadening and inspiring power in all the past history of the race. The same can be said of the "periodic law" of Newlands and Mendelejeff in chemistry, of the molecular theory of gases, of Lord Kelvin's vortex theory and of the glacial period for the establishment of the geologic antiquity of man. Nothing can be brought from all the past to compare for one moment in direct application to the relief of man's estate (which Bacon put as one of the great ends and objects of productive science)—nothing, I say, can be put in comparison with the discovery of anesthetics ; while by the discovery of antiseptics in surgery the name and fame of Sir Frederick Lister will grow to the last syllable of recorded time.

The mobilization of man and giving to him the freedom of the globe,— the railways and the steamships of our century,—are absolutely without elements of comparison in all that the past has left us. There are, however, three of the inventions and discoveries that we have inherited from the past and that have been already named, two of them coming from some distant and unrecorded centuries and one from the darkness of the Middle Ages, which have proved so indispensable to all subsequent advances that it is impossible for even the nineteenth century to present anything that can be properly compared to them. I refer to the alphabet, to Arabic numerals, and to the printing press. To this list might be added language and the use of fire. The factors I have named are presupposed by all modern progress. As I have before said, the nineteenth century is the century of science ; and it is science,—thus far mainly physical science,--that constitutes the proper object of this Association. Our

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geographical name is wide, but the scope of the Association is wider still —it deals with, and is devoted to, science,—science, which is the best product of the best powers of the human mind, the human mind, created in the image of God and inspired,—shall I not say divinely inspired,—to interpret this wonderful world, this wonderful universe? This Association marks a stage already reached in this interpretation ; but in its very title it indicates that the work is incomplete, it is still in progress. Its founders, fifty years ago, clearly saw that they were in the early morning of a growing day. The most unexpected and marvelous progress has been made since that day ; but as yet there is no occasion and no prospect of occasion to modify the title. We are still laboring for the advancement of science, for the discovery of new truth. The field, which is the world, was never so white to the harvest as now; but it is still early morning on the dial of science. It is possible that we could make ourselves more interesting to the general public if we occasionally forswore our loyalty to our name and spent a part of our time in restating established truth. Contributions to the advancement of science are often fragmentary and devoid of special interest to the outside world ; but every one of them has a place in the great temple of knowledge. The wise master-builders, some of whom appear in every generation, will find them all and use them all at last, and then only will their true value come to light.

We welcome our hosts to our meetings and discussions. We cannot prove that all will be found equally interesting ; but occasionally, perhaps, conflicting views may give rise to animated debate in which human nature asserts itself so strongly and so naturally that the debate would prove interesting even if it were carried on in an unknown tongue. (Applause.) Gratefully recognizing, then, the arduous and efficient labors of the gentlemen and ladies of Columbus who have been represented by you, and particularly the ladies of Columbus in providing so generously for our entertainment, and thanking you again for these words of welcome, I now declare the forty-eighth meeting of the American Association open for the transaction of its legitimate business.

The GENERAL SECRETARY read a resolution of the Council that members of the Local Committee, members of the affiliated societies, and residents of Columbus and vicinity should be admitted to associate membership for this meeting on the usual terms.

He also read the following report of the COMMITTEE ON ASSOCIATION LIBRARY.

The Special Committee on the Association Library begs leave to report as follows :

The regular exchanges and gifts of books for the past year have been duly transmitted to the librarian of the University of Cincinnati, and have been carefully classified according to the decimal system.

The binding of the unbound portion of the library is being steadily carried on, and in a short time will be completed.   All volumes are bound with morocco backs and corners, and bear A. A. A. S. on the top of the back.

The commodious and well-appointed new library building of the University of Cincinnati, is now in course of construction.   It will be not only fireproof, but dust-proof, and will furnish an exceptionally safe and desirable home for the collections of the Association.   Ample provision is made for their accommodation.   They will be transferred to the new building early in 1900.

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Respectfully submitted,

ALFRED SPRINGER, *Chairman*,
THOMAS FRENCH, JR.,
AMOS W. BUTLER,
T. H. NORTON,
WM. L. DUDLEY.
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June 15, 1899.

The PERMANENT SECRETARY read the names of members and fellows of the Association deceased since the Boston Meeting, among whom were the names of two honored past presidents of the Association, Prof. O. C. MARSH, of Yale University, and Prof. DANIEL G. BRINTON, of the University of Pennsylvania.

The amendments to the Constitution, duly acted upon by the Council and submitted to the General Sessions at the Boston Meeting, were read and adopted.

The Local Secretary, representing the Local Committee, made announcement of the arrangements made for the reception and entertainment of the Association.

After agreement on hours of meeting, as printed in the preliminary program, the General Session was declared adjourned.

At the meetings of the General Sessions on Tuesday, Wednesday, Thursday, and Friday mornings, in addition to the usual announcements, the following business was transacted.

It was announced that Mr. C. L. MARLATT, of Washington, had been elected Secretary of Section F, in place of Dr. F. W. TRUE, resigned; also that Dr. E. W. SCRIPTURE, of New Haven, had been elected Secre-

tary of Section H in place of G. A. DORSEY, resigned.

It was announced by the GENERAL SECRETARY that in accordance with the report of the Committee on Grants, the Council has awarded grants as follows :

1. Fifty dollars to the Committee on the Quantitative Study of Biologic Variation.

2. Fifty dollars to the Committee on the Study of the White Race in America.

3. One hundred dollars to Dr. C. H. Eigenmann for the purpose of stocking pools with different species of blind invertebrates where they may be reared and studied in the light.

It was announced that Section H has been duly authorized to hold a meeting during the winter holidays at such place as the Sectional Committee may decide ; an appropriation not exceeding $25 from the current funds was allowed said section to cover the expense of printing.

The announcement was made that by resolution of council the vice-president and secretary of any section, may, with the approval of the president and permanent secretary, arrange for joint meetings with affiliated societies at the meeting of 1900.

Cordial invitations were received from several cities, including New York and Denver. It was thought that both of these places offered special advantages for meetings of the Association, and it was accordingly decided to meet in 1900 at Columbia University, New York, beginning Monday, June 25, and to recommend that the General Committee for next year accept the invitation from Denver for 1901.

The following amendments to the Constitution, having been duly considered by the Council, were presented before the General Sessions, to be acted upon at the next meeting of the Association :

ARTICLE IX. Change the second clause from " These, with the exception of the Permanent Secretary, shall be elected," etc., to " These, with the exception of the Permanent Secretary and the Treasurer shall be elected," etc.

Change clause from " The term of office of the Permanent Secretary shall be 5 years," to " The term of office of the Permanent Secretary and of the Treasurer shall be 5 years."

ARTICLE XXII. Insert after " I, Social and Economic Science," the following, " K, Physiology and Experimental Medicine."

The following reports were presented :

REPORT OF COMMITTEE ON U. S. NAVAL OBSERVATORY.

A conference of astronomers and astrophysicists was held at the Har-

vard College Observatory in August, 1898. Careful attention was given to the present condition of the U. S. Naval Observatory, and a committee was appointed to consider what steps could be taken to increase its efficiency. The following week, the American Association for the Advancement of Science met at Boston, and the undersigned were appointed a committee to cooperate with the Committee of the Astrophysical Conference. This cooperation has been secured by making the same person chairman of both committees. The views of the other two members of the American Association Committee are given in *Science*, Vol. XIII, pp. 469 and 470. The most important result accomplished, however, has been the presentation of the matter to the Secretary of the Navy, and the appointment, by him, of a Board of Visitors, with the advice and approval of the Superintendent of the Naval Observatory. The members of this Board are, Senator WM. E. CHANDLER, Representative ALSTON G. DAYTON, Professors GEORGE E. HALE, GEORGE C. COMSTOCK, and EDWARD C. PICKERING. The Board has already held one meeting in Washington, and is now actively at work. It is believed that an opportunity is thus afforded to the astronomers of America to present their views in a form in which they will receive careful attention.

Respectfully submitted,

EDWARD C. PICKERING, *Chairman*,
T. C. MENDENHALL,
R. S. WOODWARD.

REPORT OF THE COMMITTEE ON QUANTITATIVE STUDY OF BIOLOGIC VARIATION.

Harvard University, Cambridge, Mass.
August 18, 1899.

To the Council of the American Association for the Advancement of Science, Columbus, Ohio.

GENTLEMEN :

I have a report on the grant made me to aid Mr. C. C. Adams on the quantitative study of variation. The money was to be used in collecting shells of the genus Io inhabiting the drainage area of the Tennessee river. This genus is selected because it illustrates species of differentiation associated with geographical distribution. The plan is to collect 200-300 individuals from each of several localities, noting physiographic conditions, and to measure the individuals collected.

Mr. C. C. Adams started for the headwaters of the Tennessee river— the Clinch and Guest rivers in Southwestern Virginia, July 25, 1899. A letter dated August 13 states that starting at the headwaters of the Clinch and of Guest rivers no Ios were found. Further down on the Clinch, at St. Paul, Va., 200 Ios were collected. None were found in the Guest

river at any point. These headwaters had never been explored for Ios which are more abundant lower down. At date of writing, Mr. Adams was starting for Clinton, Tennessee.

Besides car-fare to Virginia, about $22, Mr. Adams has had heavy expenses in having a boat built and hiring a guide. He believes, now, that he is on the ground, that the exploration of the Io territory will take an additional summer. Mr. Adams cannot afford to meet any large expenses and the present trip will, he estimates, cost him $100 besides the grant of the Association and the little sum I added to that grant. The problem is of great importance from the standpoint of the origin of species as well as the laws of variation. I hope that the Association may be able to make an additional grant of $50 to enable Mr. Adams to complete his work next year.

I would suggest and recommend (1) The appointment of a Committee on the Quantitative Study of Variation. (2) The granting to this committee of $50 to aid Mr. Adams in studying the variation of the fresh-water genus of Molluscs, Io, in the Tennessee river basin.

Respectfully submitted,
CHAS. B. DAVENPORT.

SEVENTEENTH ANNUAL REPORT OF THE COMMITTEE ON INDEXING CHEMICAL LITERATURE.

The Committee on Indexing Chemical Literature respectfully presents to the Chemical Section its Seventeenth Annual Report, covering the twelve months ending August, 1899.

WORKS PUBLISHED.

A Select Bibliography of Chemistry, 1492–1897. By Henry Carrington Bolton. *First Supplement.* Smithsonian Miscellaneous Collections 1170. City of Washington, 1899. pp. x+489. 8vo.

This work includes titles omitted in the volume published in 1893, and brings the literature down to 1897, the plan and scope being the same. In collecting the 5554 titles the author had the cooperation of eminent men of science and letters in several parts of the world.

Index to the Literature of Thallium, 1861–1897. By Miss Martha Doan.

This is passing through the press and will probably be issued before this Report is presented to the Section. It will form one number of the Smithsonian Miscellaneous Collections.

WORKS IN PREPARATION AND REPORTS OF PROGRESS.

Index to the Literature of Zirconium. By A. C. Langmuir and Charles Baskerville.

The manuscript has been critically examined by each member of the

Committee and unanimously recommended to the Smithsonian Institution for publication.

A Select Bibliography of Chemistry, 1492–1897. *Second Supplement.* By Henry Carrington Bolton.

The manuscript of this work, which embraces Chemical Dissertations only, has been completed and presented to the Smithsonian Institution.

A Bibliography of Malonic Acid and Its Derivatives has been begun by Dr. Thomas Clarke, University of North Carolina.

A Third Supplement to the Select Bibliography of Chemistry has been commenced by Dr. H. Carrington Bolton.

"*Science Abstracts.*" The Physical Society of London published in 1895–1897 three volumes of "Abstracts of Physical Papers from Foreign Sources," edited by Mr. J. Swinburn. Each volume has author and subject indexes. Since January, 1898, these abstracts have appeared monthly in enlarged scope under the title : "Science Abstracts. Physics and Electrical Engineering." Issued under direction of the Institution of Electrical Engineering and the Physical Society of London. J. Swinburn, Editor. Monthly. Spon and Chamberlain, Agents, New York.

This important undertaking includes electrochemistry and chemical physics; the Editor is assisted in his work by W. R. Cooper and a corps of 52 abstractors.

Committee {
H. CARRINGTON BOLTON, *Chairman*,
F. W. CLARKE,
A. R. LEEDS,
A. B. PRESCOTT,
ALFRED TUCKERMAN,
H. W. WILEY.

The closing General Session was held in the University Chapel at 3.30 P.M., Friday, August 25, PRESIDENT ORTON in the chair.

The General Secretary read letters from MR. EMERSON McMILLAN, presenting $1,000 to the Association. The announcement was made that the Council had made Mr. McMillan a patron of the Association.

The Permanent Secretary then presented his financial report, which is elsewhere printed in full, and read a running account of the features of the Columbus meeting.

Ex-President MENDENHALL, Chairman of the Committee on Resolutions, spoke as follows :

The sessions of the forty-eighth meeting of the A. A. A. S. are now about to come to an end, after having carried out a program well filled

with papers and other communications, many of which are of more than usual significance.

The work of the general Association and of its several sections has been greatly facilitated by the extremely satisfactory arrangements for the comfort, convenience, and pleasure of its members, which the local authorities have taken care to provide. It is but just to say that in some important respects these arrangements have been unique in the experience of the Association, and in all respects they have been so well conceived and carried out that those who have been benefited thereby are quite unwilling to allow the meeting to close without special expression of their appreciation of what has been so generously done for them.

To this end the following resolution has been offered for adoption in the general session :

RESOLVED, That the members of the A. A. A. S. who have attended the meeting in Columbus desire to have as a part of the official record of that meeting an earnest expression of their appreciation of the most courteous reception and generous welcome which has been extended to them by all who have been locally interested in this meeting, and that they hereby tender their cordial thanks as follows :

To his Excellency, Governor Bushnell, for the freedom of the great State of Ohio, so eloquently offered by his representative, General Axline.

To His Honor, Mayor Swartz, for the very suggestive and delicately worded offer of protection, police and otherwise, to all members of the Association while within the limits of the Capitol City, which his representative, Judge M. B. Earheart, so happily conveyed, along with other words of wit and wisdom, and as well to the Columbus Board of Trade and other municipal authorities and organizations.

To the Board of Trustees, President, and Faculty of the Ohio State University, for so generously placing at the disposal of the Association the entire equipment of that institution, including buildings, appliances, and the beautiful grounds on which the University is located, as well as a large measure of their own time and energy, both before and during the meeting.

To the President and Mrs. Thompson and the Local Committee for the most pleasing reception extended to members on Monday evening at the New Chittenden Hotel.

To the officers and members of the Columbus Club, for the charming reception on Thursday evening.

To the Ladies' Reception Committee for the many delicate attentions bestowed upon the lady members and visitors revealing, in fact, new possibilities in the way of arrangements for their comfort and pleasure, especially for the opportunity afforded them to see something of the city on Tuesday evening, and on Thursday afternoon, and for the beautiful concert and charming Garden Party of Wednesday afternoon.

To the Pennsylvania railroad lines, the Hocking Valley Railroad, the Toledo and Ohio Central Railroad, and the Columbus, Sandusky, and

Hocking Railroads for generous arrangements in the matter of excursions.

To the Central Union Telephone Company for the free use of its lines.

To the Columbus Natural Gas and Fuel Company for the beautiful display of the resources of its territory.

To the State Archæological Society for its hospitalities at Fort Ancient, and to Mr. E. O. Randall for conducting the excursion ; especially to Mr. R. M. Haseltine, State Mine Inspector, for arranging and conducting the excursion to the coal fields ; and particularly to the people of Lancaster for the unexpected, but most generous entertainment offered to and accepted by those members of the Association who made the excursion of Thursday.

And finally and more than to all others, to the Local Committee which, under the most efficient direction of its Chairman, Hon. Henry C. Taylor, and its Secretary, Professor B. F. Thomas, has been untiring in its efforts to insure the success of the meeting.

That they have completely and satisfactorily accomplished this result every member present will testify.

The following letter from ex-President E. S. Morse was then read by Dr. Mendenhall.

Columbus Club.

My Dear Mendenhall :

It is with great regret that I am forced to return before the meeting adjourns and my regret is mainly because I cannot join with you all in the acknowledgment and grateful recognition of the truly royal way in which the citizens of this beautiful city have welcomed the Association.

We have had many meetings in this great country west of the Alleghenies but I must say that never before have all the smaller comforts of the members been so delicately looked after. The carriages provided. the unexampled conveniences of the sessions, the hearty and delicious lunches, the receptions. In fact in every way it has been, to use a slang expression, gilt-edge throughout. And it all has been done in so quiet and unobtrusive a way that the members have been as much at ease as in their own homes. I wanted to stay and say all this, but it has been simply impossible and so I beg that you in some way convey to the Local Committee my heartfelt acknowledgment of all they have done for us.

Faithfully yours,
Edw. S. Morse.

The resolutions were seconded by Prof. C. E. Bessey, Mr. Elihu Thomson, Prof. E. W. Morley, Mr. John Hyde, Mrs. S. H. Gage, and

Mrs. ESTHER HERMANN, Prof. J. A. HOLMES and Prof. R. S. WOOD-WARD.

The resolution was unanimously adopted.

The Association then adjourned until Monday, June 25, 1900.

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FREDERICK BEDELL,

General Secretary.

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REPORT OF THE PERMANENT SECRETARY.

The first Columbus meeting of the Association marked the beginning of the second half century of its existence, and marked it in a most encouraging manner. While the attendance was not as large as in some years, yet it exceeded not less than thirty-three of the forty-eight meetings which the Association has held. The total registration was 353, and twenty-eight States, the District of Columbia, and Canada, were represented. The State which had the largest representation was Ohio with 113 members and associates. The others followed in the following order: New York, 52 ; District of Columbia, 25 ; Pennsylvania, 21 ; Massachusetts, 18; Michigan, 13 ; Indiana, 12 ; Iowa and Illinois, 9 each ; Kentucky, 7 ; Canada, Connecticut, Kansas, New Hampshire, Nebraska and Wisconsin, 6 each ; Minnesota, Texas, and Maryland, 5 each ; Missouri and Virginia, 4 each ; West Virginia and North Carolina, 3 each ; New Jersey and Colorado, 2 each ; and Alabama, Rhode Island, Washington, and Louisana, 1 each.

Including the papers read before those of the affiliated societies whose meetings were held jointly with related Sections (excluding, however, those papers read before affiliated societies at their independent meetings), there were presented 273 papers. Of these, 14 were read before Section A (and these include five reports of progress, an especially valuable feature of the work of this Section this year, and one which ought unquestionably to be adopted by other Sections); 40 were read before Section B ; 55 before Section C ; 15 before Section D ; 33 before Section E ; 19 before Section F ; 33 before Section G ; 27 before the Botanical Club of the Association ; 20 before Section H ; and 17 before Section I.

Nine affiliated societies met with the Association during the week. The Geological Society of America, the American Chemical Society, and the Botanical Society of America held their meetings jointly with Sections E, C, and G, respectively ; and it is expected that at the next meeting of the Association the American Mathematical Society and the Astronomical and Astrophysical Societies will adopt the same course toward Section A, and the Society for the Promotion of Engineering Education towards Section D.

The American Microscopical Society this year returned to its affiliation to the main Association, after an absence of a number of years, in so far as that it held its meetings at Columbus on the 17th, 18th, and 19th of August.

A number of constitutional amendments were adopted as printed in the Boston volume, and it should be pointed out that one of the most important of them looks toward a greater continuity in membership in the Council, thus making it a body of men with some definite experience in the management of the Association and familiarity with its business affairs. Other important constitutional amendments were introduced and will be acted upon at the next meeting. These are printed in full in the report of the General Secretary.

The PERMANENT SECRETARY reported to the Council at its meeting held in New York in December, 1898, that he had given up the office in Salem, and discharged the janitor. This was done October 1, 1898, and will result in a saving to the Association of $244.00 per annum. The property of the Association contained in its Salem office was transported largely to the residence of the Assistant Secretary, at North Andover, Mass. During publication of the Boston volume, the Secretary secured bids for the printing of the proceedings of the Association from a number of firms located within comparatively easy reach of Washington, and finally entered upon a contract, by the authority of the Council, with a firm accustomed to handle scientific work and which will print the annual volume at a rate which will result in a very considerable saving of the Association funds.

Of the 122 members elected since the Boston meeting, 96 have perfected their membership, and also 13 of those elected at the Boston meeting, and 2 elected at the Detroit meeting; 31 have paid their arrears, and 1 name omitted from the Boston list by mistake is now printed in the Columbus volume, making 143 names added to the roll since the Boston volume was published.

From the Detroit list, 27 names (including one patron and two life members) have been transferred to the list of deceased members; 28 members and fellows have resigned; and 122 have been omitted for arrearages, making a deduction of 177 from the list.

Sixty-four members have paid fellowship fees and have been transferred to the list of fellows; 3 have paid fees of life membership; and one life fellow has been elected by vote of the Council.

One member has become a patron, and the name of one corresponding member has been transferred to the list of honorary fellows.

The following is a comparative statement of the roll as printed in the Detroit and Boston volumes, and in the present volume:

	Detroit.	Boston.	Columbus.
Living patrons	2	2	2
Corresponding member	I	I	.
Members	844	949	897
Living honorary fellows	3	I	2
Fellows	760	776	794
	1610	1729	1695
Honorary life members (founders) included in the above	6	6	6

This result is by no means discouraging considering the especial efforts which were made for the success of the Boston meeting, and the large number of new members brought in at that time ; and it must be remembered that the figure 1695 represents a perfectly paid up and interested membership. Moreover, it is probable that between the date when the list is closed and the close of the year enough more members will have paid up to reach the Boston figure.[1]

The distribution of publications since the last report is as follows : Proceedings, Volumes I to XLVI, delivered to members, 226 ; sold, 28 ; exchanges, 81 ; presented, 18 ; duplicate copies to members, 2, making a total of 355.

Proceedings, Volume XLVII, delivered to members, 1556; sold, 33 ; exchanges, 254 ; presented, 7, making a total of 1850. Returned by exchanges, 2. Copies of Constitution sold, 5.

The financial report of the Permanent Secretary will follow that of the Treasurer and the Retiring Permanent Secretary.

The Permanent Secretary wishes to acknowledge for the Association the courtesy of D. Appleton & Co., in loaning the Association the line cuts illustrating the address of Vice President Benjamin. He also wishes to acknowledge the kindness of the editor of *Science* in loaning the Association the blocks of the four full-page portraits which illustrate the same address.

L. O. HOWARD,

OCTOBER 30, 1899.   *Permanent Secretary.*

REPORT OF THE TREASURER.

In compliance with Article 15 of the Constitution, and by direction of the Council, I have the honor to submit the following report, showing receipts, disbursements, and disposition of funds of the Association for the half year ending December 31, 1898.

Receipts have come into the keeping of the Treasurer from two sources, namely : First, from the commutations of fees of life members of the Association, and secondly, from interest on funds deposited in savings banks. The aggregate of these receipts for the half year is $404.47.

Disbursements made in accordance with the directions of the Council amount to $50.00.

The details of receipts, disbursements, and disposition of funds are shown in the following itemized statement.

DATED APRIL 1, 1899.

[1] December 22, 1899. The permanent secretary is glad to be able to insert in the printer's proof the statement that at this date the number of members in good standing is 1,721.

THE TREASURER IN ACCOUNT WITH THE

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

DR.

1898.

December 31, To balance from last account.................. $5,729 37

To amount received from F. W. Putnam for 6
life membership commutations............... 300 00

To amount received as interest on funds of the
Association deposited in savings banks as fol-
lows :

From Cambridge Savings Bank, Cam-
bridge, Mass........................$ 6 62

From Emigrant Industrial Savings Bank,
New York, N. Y..................... 24 73

From Institution for the Savings of Mer-
chants' Clerks, New York, N. Y....... 40 10

From Metropolitan Savings Bank, New
York, N. Y.......................... 33 02
————— 104 47

Total.................................. $6,133 84

CR.

1898.

December 31, By cash paid as grant to Committee on Standards.$ 50 00

By cash on deposit in banks as follows :

In Cambridge Savings Bank, Cam-
bridge, Mass.....................$ 338 18

In Emigrant Industrial Savings Bank,
New York, N. Y.................. 1,437 98

In Institution for the Savings of Mer-
chants' Clerks, New York, N. Y... 2,482 66

In Metropolitan Savings Bank, New
York, N. Y...................... 1,684 82

In Fifth Avenue Bank, New York, N. Y. 140 20
————— 6,083 84

Total.................................. $6,133 84

NEW YORK, MAY 4, 1899.

I have examined the foregoing account and certify that it is cor-
rectly cast and properly vouched.

EMORY McCLINTOCK, *Auditor.*

F. W. PUTNAM, PERMANENT SECRETARY,

THE AMERICAN ASSOCIATION FOR

From January 1, 1898,

Dr.

To balance from last account..		$ 1563 57
Assessments for Boston Meeting	$2409 00	
Assessments previous to Boston Meeting..........	2343 00	
Admission fees previous to Boston Meeting.......	55 00	
Admission fees Boston meeting	120 00	
Associate fees.................................	75 00	
Fellowship fees................................	2 00	
Life membership...............................	300 00	
Publications sold..............................	99 97	
Receipts for binding	57 40	
General receipts	6 63	
Interest received, temporary investment..........	26 89	
		5494 89

$ 7058 46

I hereby certify that I have examined this amount

CAMBRIDGE, MASS., DECEMBER 24, 1898.

IN ACCOUNT WITH
THE ADVANCEMENT OF SCIENCE.
to August 20, 1898.

By Publications.   Cr.

On account of printing and binding 2500 copies
Proceedings, Vol. 46, Detroit volume........... $2079 68
Illustrations for Volume 46......................   2 19
Index for Volume 46 ....   12 50
Extras of addresses from Volume 46............   24 10
Extras of addresses and reports from Volume 47,
including composition of 112 pages of the Boston
volume... 167 43
Boston Pamphlet, including composition of 76
pages of Boston volume 166 70
                                                 ———— 2452 60

By Expenses, Boston Meeting.
Expenses of sections and preliminary program ... 167 22
Expense Winter Meeting of Section H, 1897........   3 00
                                                 ———— 170 22

By General Office Expenses.
Rent of office, eight months 96 00
Extra clerical labor and typewriting.............   58 82
Petty office expenses and stationery 74 35
Telegraph and telephone 8 05
Postal Guide, $2.00; Post-office box, 9 months, $6.00   8 00
Postage, $344.54; express, $251.30............... 595 84
                                                 ———— 841 06

By Salaries.
Permanent Secretary to August 16, 1898.......... 781 20
Assistant Secretary to August 1, 1898............ 420 00
Janitor to July 1, 1898..........................   50 00
                                                 ———— 1251 20

By cash paid Treasurer........................... 300 00
By balance cash on hand paid to L. O. Howard, Per-
manent Secretary 2043 38
                                                 ———— 2343 38
                                                 ═════
                                                 $7058 46

and that it is correctly cast and properly vouched for.

S. C. CHANDLER, *Auditor.*

L. O. HOWARD, PERMANENT SECRETARY,
THE AMERICAN ASSOCIATION FOR
From August 20, 1898, to

DR.

To balance received from F. W. Putnam	$2016	49
Boston assessments	1242	00
Columbus assessments	27	00
Assessments previous to Boston Meeting	564	00
Admission fees previous to Boston Meeting	5	00
Admission fees, Boston Meeting	940	00
Associate fees, Boston Meeting	627	00
Fellowship fees, Boston Meeting	74	00
Life membership fees, Boston Meeting	397	00
Publications	21	72
Subscriptions	1	50
Binding	13	50
General receipts, including interest	41	23
	$5970	44

I hereby certify that I have examined this account and that it is posit January 5, 1899, at the Citizens' National Bank of Washington, with Secretary holds the personal note of the member, payable Feb. 23, 1899.

IN ACCOUNT WITH
THE ADVANCEMENT OF SCIENCE.
December 31, 1898.

CR.

By Printing and Publication.

On account, volume 47 ·······························	$1012 00	
Wrappers, circulars, etc ·······························	39 95	
		$1051 95

By Expenses Boston Meeting.

Secretaries of sections ·······························	79 35	
Graphophone ·······························	10 30	
General expenses ·······························	118 61	
		208 26

By Office Expenses.

Rent of Salem office, August and September ·········	24 00	
Express ·······························	16 09	
Postage ·······························	32 63	
Telegrams ·······························	8 17	
Check-book and cash-book ·······························	12 02	
Letter heads and stamped envelopes ··············	31 15	
Moving office effects ·······························	54 84	
Indexing and typewriting ·······························	12 55	
Incidentals ·······························	1 08	
		192 53

By Salaries.

Permanent Secretary ·······························	468 80	
Assistant Secretary ·······························	300 00	
Janitor, July 1 to October 1 ·······························	25 00	
		793 80
By balance to new account ·······························		3723 90
		$5970 44

correctly cast and properly vouched for, and that the balance was on de-
the exception of a partial life membership fee for which the Permanent

G. K. GILBERT, *Auditor.*

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PROCEEDINGS

OF

The American Association

FOR THE

ADVANCEMENT OF SCIENCE

FORTY EIGHTH MEETING

HELD AT

COLUMBUS, OHIO.

AUGUST, 1899.